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CONTENTS

Direct Observation and Psychological Tests in Anthropological Field Work . . . . . Ivan N. Mensh and Jules Henry 461

Ecology and the Protohominids . . . . . George A. Bartholomew, Jr. and Joseph B. Birdsell 481

Some Aspects of Human Ecology in the Central Plains . . . . . Waldo R. Wedel 499

Symposium: Did Man Once Live by Beer Alone? . . . . . Robert J. Braidwood, *et al.* 515

Ethnography, Cultural and Social Anthropology . . . . . Robert H. Lowie 527

Culture and the Causes of Culture . . . . . Philip H. Bagby 535

Adoption on Mokil . . . . . J. E. Weckler 555

Two Surviving Luiseño Indian Ceremonies . . Raymond C. White 569

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CONTENTS

Direct Observation and Psychological Tests in Anthropological Field Work ..... Ivan N. Mensh and Jules Henry 461

Ecology and the Protohominids ..... George A. Bartholomew, Jr. and Joseph B. Birdsell 481

Some Aspects of Human Ecology in the Central Plains ..... Waldo R. Wedel 499

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Culture and the Causes of Culture ..... Philip H. Bagby 535

Adoption on Mokil ..... J. E. Weckler 555

Two Surviving Luiseño Indian Ceremonies ..... Raymond C. White 569

This Issue, and Others ..... pp. 480, 514, 526

Book Reviews

GENERAL AND THEORETICAL—Levy: *The Structure of Society* (Fallers), 579; Penniman: *A Hundred Years of Anthropology* (Count), 581.

ETHNOLOGY AND ETHNOGRAPHY—Evans-Pritchard: *Kinship and Marriage among the Nuer* (Schneider), 582; Soothill: *The Hall of Light* (Fried), 584; Rawlinson: *India: A Short Cultural History* (Worman), 585.

CULTURE CHANGE—Slotkin: *Menomini Peyotism* (Stewart), 586.

PHYSICAL—Boule and Vallois: *Les Hommes Fossiles, Éléments de Paléontologie Humaine* (Howells), 587.

ARCHEOLOGY—Ford: *Measurements of Some Prehistoric Design Developments in the Southeastern States* (Spaulding), 588; Griffin: *Archeology of Eastern United States* (Byers), 592; Caso and Bernal: *Urnas de Oaxaca* (Eckholm), 593; Martin, Rinaldo, Bluhm, Cutler and Grange: *Mogollon Cultural Continuity and Change, The Stratigraphic Analysis of Tularosa and Cordova Caves* (Wendorf), 594.

OTHER—Segy: *African Sculpture Speaks* (Crook), 596; Thomas: *The Salvage* (Sasaki), 597; Holleman: *Shona Customary Law: With Reference to Kinship, Marriage, the Family and the Estate* (Christensen), 599; Eliade: *Le Chamanisme et les Techniques Archaïques de l'Extase* (Bacon), 600; R. and C. Berndt: *Sexual Behavior in Western Arnhem Land* (Beach), 601.

BOOK NOTES 602

Brief Communications

A Mechanistic Theory of Human Evolution ..... N. C. Tappen 605

Notes on Two Dakota "Holy Dance" Medicines and Their Uses ..... James H. Howard 608

Time Perspective in Plains Indian Beaded Art ..... Orvoell R. Gallagher and Louis H. Powell 609

Letter to the Editor

Tschopik's *Aymara of Chucuito* ..... A. L. Kroeber 613

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AMERICAN ANTHROPOLOGIST

VOL. 55 OCTOBER No. 4

DIRECT OBSERVATION AND PSYCHOLOGICAL TESTS IN ANTHROPOLOGICAL FIELD WORK

By IVAN N. MENSH and JULES HENRY

I

IT SEEMS to the writers that in order to make general observations about the use of projective psychological tests in anthropological field work we first need to make some statement about the goals of cultural anthropology. Such a statement would appear to be indispensable, since psychological investigation in anthropology ought to be considered, like physical anthropology, archeology, and linguistics, one of the roads that leads ultimately to the common goal—the global understanding of man.

Anthropology aims at the understanding of man, through elucidation of biological, psychological, historical, and social processes. In "biological" we include physiological function as well as physical differentiation (racial differentiation and evolution). By "historical" we understand the dynamics of interlocking event-sequences involved in cultural changes. By "social" we understand social structure and the dynamics of social process. By "psychological" we mean the processes of concept-formation, learning, personality development, perception, etc.; in short, all the events that take place inside individuals' minds as functions of outer events impinging on a neural network. It is clear that this aim is comprehensive and complex, and is not exclusively anthropology's. Other disciplines are concerned with the same things, and, like anthropology, are confronted with the same problem, of a constantly expanding universe of research, and increasingly unmanageable data. However, the reality is clear: anthropology, like all the sciences of human behavior, has the problem of vanishing disciplinary boundaries, and the interests of anthropologists increasingly embrace the areas of research just outlined.

Where do psychological tests fit into the aim of global understanding? Let us look at the following model:

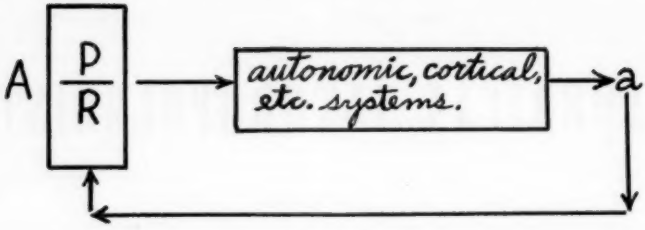

FIG. 1

Here the model sets forth the argument that in the presence of stimulus configuration A, which we call, *for the moment*, and as an illustration only, a certain constant ratio between reward and punishment, operating in a relatively homogeneous cultural environment, the autonomic, cortical and other mediating systems of the individuals in the culture will be relatively permanently conditioned to response "set" *a*. It will readily be seen that the model brings together, in an ideal, but in a relatively plausible way, three of the important dimensions of research we have been concerned with in anthropology: the biological, the cultural and the psychological, for culture and the structure of society determine the character of the ratio $\frac{P}{R}$; and its effect on the autonomic nervous system is clearly a matter to be understood in terms of the biology of man and the structure of his nervous and other organic systems. There is implied, however, in the model, another biological consideration: the process of the evolution of man; for since culture, acting on the organism, brings about certain characteristic psychosomatic sets—like the tendency to dominate, to submit, or to develop hypertension, asthma, or gastro-intestinal, or other psychosomatic disorders—it is clear that culture, through its instability, plays an important role in the bodily changes that take place in man over time. [Thompson (1951: 252) used the expression "psychosomatic sets" for the action of core values on the organism.]

Thus the role of psychological investigation in anthropology is an important one, for it not only enables us to understand what happens when certain environmental phenomena impinge on the psycho-physiological processes of human beings, but it also helps toward an understanding of the role culture has played in physical evolution.

The Role of Direct Observation: The immediate issue for the anthropologist is to arrive at the best possible understanding of A in Figure 1. In other words, the first step is to understand the stimuli that impinge on the organism. If, for example, there exists in any society a relatively constant ratio of rewards to punishments, then what are the rewards and punishments and precisely

what are the quantitative relations between the two? Obviously there is one good way we can learn the answers to the questions: we must watch reward and punishment being administered. This brings us to the crux of the matter as far as psychological tests are concerned: tests tend to measure *a*—the *end results* of the conditioning process—rather than the *conditioning events*. On the other hand, even when they do, as in the TAT, for example, pick up something of the conditioning process, the data are drastically condensed: the whole punishment process, for example, may be summed up in a response like "he was punished," and the crucial details are left out. Furthermore, the time dimension is not stated. That is to say, we learn little about frequency of punishment or the lag between act and the punishment for it. Thus if we have *only* psychological tests on our subjects we frequently have to guess how the subjects came to be the way they are. Thus tests often tell us little or nothing about many of the crucial details of the way in which the autonomic and other organic systems of the population are impinged upon by the socio-cultural process.

There are only two ways in which a human being can observe a phenomenon: he can watch it with the naked eye, or with an instrument. In each case interpretation of what is observed must take place in order to make the observed phenomenon have meaning to the observer. However, in the second case the accuracy of the instrument is always in question. Hence it can be argued that at the present time in the sciences of human behavior direct observation is at least as accurate as trans-instrument observation until it can be shown that instruments are more precise and more comprehensive than the scope of direct observation. The instruments presently in use are imprecise, as proved by the revision that is constantly going on; and it would be a rare clinical psychologist who would argue that personality evaluations derived from tests are more valid than those based on extensive interviewing and observation of the subject. An instrument like the TAT has often been used in anthropological field studies without objective devices for interpreting the responses to it, and thus may be entirely dependent on individual variations among examiners. It ought to be pointed out, meanwhile, that while he who observes directly has only his own past experiences to deal with in interpreting his data, whoever uses an instrument must employ not only his own past experiences, but also the past experiences of the instrument.

Interpretation of personality on the basis of direct observation of the human organism functioning in its native habitat has sometimes been called "impressionism." In the first place it seems necessary to indicate that these interpretations have never been proved to be false. In the second place the recent paper by Henry and Spiro (1953) has shown that there is a close correspondence between anthropologists' direct observations and psychological test data.

An objection to direct observation has been that the observer, by his mere presence, biases the situation he is observing, hence what he sees is not "reality" as it actually occurs in nature, but a reality distorted by the presence of the observer. We believe that in anthropology we must accept this as a constant possibility, but we cannot for this reason stop observing; nor can we surround our cultures with one way screens. What can be done in anthropology to reduce distortion, is to remain with the groups studied long enough to repeat observations many times and in different contexts. This also permits the observer to become more of a participant observer, and less of an external factor introducing uncontrolled variation.

If we wish to determine whether a given child is anxious or not, we can: (a) watch him with his parents; (b) watch him with other children; (c) watch him in general gatherings; (d) talk to the child; (e) have the child observed by more than one anthropologist; (f) ask the people around the child whether he is upset; (g) give the child a projective test. We can watch the child getting up in the morning, eating, bathing, and even watch him while he sleeps, in order to see whether he has night-terrors or other symptoms of anxiety. It is common knowledge nowadays that error can be reduced through multiplying observers and by multiplying the number of points of view from which the phenomenon is observed. It should be borne in mind that the critics from outside anthropology who feel that anthropologists' reports on personality are like those of a tourist, do not fully appreciate the increase in reliability gained through the opportunity to observe systematically subjects in a great variety of life situations. The anthropologist who spends a year in the field in "participant-observer" field work can have some of his subjects under observation over relatively long periods of time, come to see them functioning in relation to a complex milieu; and in addition may also obtain from his subjects accounts of how they view their own life's experiences. Thus it seems to us that some of the misunderstanding about anthropologists' evaluations of personality may stem from lack of appreciation of this variety in the sources the anthropologist may tap during the course of participant-observer field work.

The anthropologist who spends a year in the field has at least some of his subjects under constant observation, can study the entire milieu, and can *also* obtain the subject's account of his experiences.

When we come to the clinical psychologist we see that in a good clinical psychological work-up the psychologist spends only a few hours with the subject and generally sees him but once or twice. To be sure, he attempts to compensate for the difficulties by varying the stimuli throughout a range, thus increasing the number of positions he takes as observer, and the number and range of the reactions the subject must make, but nevertheless he still lacks the advantages the anthropologist has in having subjects under long-time observation.

In all of this, of course, there is implied no derogation of the methods of clinical psychology which is adapted to the purposes for which it is needed in the clinical situation in our culture. All we intend to point out is that anthropological observation is not as vague as has been thought, but on the contrary, has features to commend it to those who rightly insist that good observations are the first requisite for a respectable science.

One of the obvious results of the use of instruments for the study of the universe is that we perceive only what the inherent design of the instrument permits it to transmit. A pair of eight-power binoculars permits us to see objects at a certain distance, but a pair of sixteen-power binoculars will permit us to see objects much farther away. It is also true that with binoculars objects close to us will be blurred. An instrument is always a mixed blessing. When we come to the use of psychological tests, however, we deal with phenomena of much greater complexity than binoculars. Binoculars do not have to reduce the universe to specific theoretical categories. All they are "asked" to do is to transmit light rays, and no one asks them to do it in terms of the wave or the corpuscular theory of light. But a projective test like the Rorschach has an interpretive system, structured in definite categories, in terms of which personality *must* be perceived. It is as if looking through a pair of binoculars one *had* to see the universe in terms of either the wave or the corpuscular theory of light. Now, since the Rorschach categories are organized in a special way, it means that as long as we use the test we will perceive people in terms of Rorschach theory. Thus the instrument we use to look at the universe determines what we shall see. As long as we use the same instrument we will see only what it transmits to us. In this way we build for our minds a prison out of the very instruments that were designed to set them free. This, of course, has been the fate of theories and scientific apparatus throughout the history of science.

In this situation direct observation enables us to discover relationships among phenomena that the projective test used alone might not permit us to do. It is true that the eye itself is not untrammeled by theory and by preconceived categories, for we bring to any situation biases determined by previous experience. But at least, so it seems to us, when we use direct observation we have not double-locked the door of science by using a "standardized" eye that will look in only one direction (Luchins 1952).

It is important to note that the above discussion applies to the use of psychological tests in anthropological field work, and specifically to studies in which no direct observations on the subjects accompany the projective test data, for obviously the criticism that the instrument limits what shall be seen does not apply strictly where direct observation enters into the study.

The quantitative problem. The issues here are: (a) the quantification of direct observations; (b) the obtaining of enough cases to lend credibility to the data on personality.

(a) It will possibly remain forever a matter of taste whether in describing human behavior one wishes to use numbers or not. In the present discussion, however, the authors accept the orthodox scientific position that understanding the universe is closely related to our capacities to describe it in quantitative terms. We also assume that since human life is made up of experiences and events, the frequency of the occurrence of these experiences and events can be counted. The question arises, can one reduce to quantifiable terms the enormous variety of human experience? The answer to this question is as follows: most of the things the anthropologist sees can be described. What we see, however, and the power of accurate description are functions of the developmental stage of science at the moment. Hence we can always observe and describe within the limits of our scientific achievements. The next step depends on the selection of units for quantification. If we cannot find units it means that we have not reached the stage in science where we are ready for quantification. But it is also a shrewd guess that if we cannot find units we cannot give a description of the phenomena we see.

The quantification of direct observations depends therefore on the state of the human sciences at the moment and our capacity to discover units. The task of the field anthropologist becomes, therefore, the selection of units of observation. We give below two examples, one from our culture and one from the Pilaga (Henry and Boggs 1952: 266–268), of types of units that may be selected for the study of psychological characteristics, on the basis of direct observation:

TABLE I*

NUTRITIONAL BEHAVIOR OF FOUR PILAGA INDIAN CHILDREN

	Yorodaikolik (4 years)	Tapani (8–9 years)	Naicho (6 years)	Deniki (15 months)
1. Given food	9	7	27	42
2. Deprived of food	7	0	2	4
3. Deprivation followed by restitution	1	0	1	2
4. Attempt made to deprive of food	2	1	1	4
5. Gives food	8	7	1	3
6. Withholds food	6	5	2	3
7. Deprives others of food	2	6	1	0
8. Attempts to deprive others of food	1	5	0	1
9. Receives part of a whole	0	1	0	4
10. Punished while eating	0	0	0	3

* The total number of observations, including nutritional behavior, for each of these children is as follows: Yorodaikolik, 190; Tapani, 207; Naicho, 238; Deniki, 208.

SUMMARY OF TABLE I

	Yorodaikolik	Tapani	Naicho	Deniki
Subject Given Food.....	9	7	27	42
Other Nutritional Behavior.....	27	25	8	24
Total Nutritional Observations.....	36	32	35	66

Table I is constructed from nearly a thousand observations, of which the following are but two examples:

The three children of Diwa'i are feeding peacefully together. Deniki, the baby, waves his hand for food and mother gives him a small piece of palm dipped in fat. After eating a second piece he is given the breast.

Deniki, Naicho and Soroi are together. Deniki is holding a dish with a very small quantity of cooked fruit in it. Soroi says, "Share it with me," and takes one fruit out of the dish. Naicho immediately snatches another one away violently, but not before Deniki has already taken one out, which he then offers to Naicho, appearing not to comprehend her action. (Henry and Boggs 1952: 265)

It will be seen that the units "given food" and "deprived of food" are clearly discernible in these matter-of-fact descriptions of what transpired before the eyes of the anthropologist, planted near the situation with his open notebook. Obviously, however, other gross units can be discerned also. For example, in the first observation the following additional units can be separated out: (1) relatively conflict free feeding situation; (2) in the presence of all the children, mother feeds only baby with delicacy; (3) baby asks for solid food of mother and is given it; (4) solid food alternated with breast.

In the second observation the following additional units can be isolated from the total matrix: (1) use of the expression "Share it with me" accompanied by forcible deprivation of infant of food; (2) enforcement by peer-group (i.e. other children) of cultural imperatives; (3) mass attack on food by other children, i.e. three against one; (4) apparent effort of infant to proffer food "voluntarily."

Table II is constructed from fourteen 600-word protocols of observations of the 45-minute feeding periods of neo-nates with their mothers in a maternity hospital. The following is an abstract from one such protocol:

The mother was lying on her back in a slightly raised position when the nurse carried the baby in and laid it on the bed beside the mother. The mother took the bottle and put the nipple in the baby's mouth at 0-00. "Every time he comes in here he's asleep." Baby moved arm. Mother took nipple out at 0-05.

After two attempts the nipple was successfully inserted at 0-47. "He's not doing anything. Can they when they're asleep?" Observer replied, "Not very well." As baby

TABLE II

FREQUENCY OF UTTERANCES OF MOTHERS TO THEIR BABIES, EXPRESSED AS PERCENTAGES OF TOTAL NUMBER OF UTTERANCES COUNTED (N=472)*

Utterance	Percentage
1. Utterances containing the expression "Come on"	32
2. "Wake up" or some related phrase	27
3. Utterances pertaining to quantity or speed	22
4. Utterances in which the adult's wishes are opposed to the baby's wishes	15
5. Utterances pertaining to achievement, work, failure, or success of the baby or the mother	11
6. Miscellaneous	13

* Not all utterances were counted because some of them had to do with matters not immediately relevant to this study. It is to be noted that since many utterances were coded under more than one category, the percentages add up to more than 100. Not all categories are listed in the table. Categories not listed have very low frequencies.

started sucking mother said: "There he goes." Baby sucked from 1-23 to 1-27. Mother jiggled nipple and patted baby's buttocks. Baby moved arms. "They have a hard time opening their eyes." Mother jiggled baby's arms. "Oh, sleepyhead." Mother jiggled nipple and baby moved arms. "Come on." Mother laughed. Mother patted baby's back and pulled baby's arm away from his face. Mother patted baby's hair, said, "Come on," and shook baby. "He just doesn't want to wake up." (Henry and Boggs 1952: 268)

It will be seen at once that several of the types of verbalization presented in the table appear in the protocol. Other units that might be isolated with relative ease for purposes of analysis are: (1) Role of the nurse in the feeding situation; (2) average length of sucking time; (3) motility patterns of mother, e.g., patting, rolling, pinching, holding, controlling baby; (4) motility patterns of baby, e.g., relative vigor of sucking, position of hands.

(b) One of the major contributions of the use of psychological tests in field studies lies in the opportunity to increase the range of sampling with systematic, standardized observations requiring relatively brief periods of administration. However, in statistical treatment of data from projective tests it should be recalled that these tests were designed for study of the individual case, and only later adapted for group use. The problem therefore arises (Mensh 1950) that the distribution of scores and of scoring categories of the projective tests frequently are not consistent with the assumptions of statistical analysis designed for the usual group data of the so-called psychometric techniques, i.e., the traditional-type psychological measures. For example, normal distributions of test scores are assumed in the usual statistical method of analysis, yet projective test data, as from the Rorschach, seldom meet this

criterion, e.g., the range of M responses is 0–20+, mean of 3.50, standard deviation of 3.24, skewed with "the vast majority . . . massed in the 0–2 range." (Beck *et al.*)

The projective test as a check on direct observation. The argument for the use of the projective test as a check on the observations of the anthropologist has been advanced on a number of grounds. The most usual argument is that observations tend to be biased because of the anthropologist's own past experience, while psychological tests are not biased. Thus the test is conceived to be a kind of mechanism without heart and without past experience. Some discussion of this has already been given. We may now consider other aspects of the problem. In the first place, the fact that Henry and Spiro (1953) found that generally the test results support the anthropologist's findings based on direct observation might readily be taken to prove not that the anthropologist was correct in the first place, but that contamination phenomena are present in the observer-test experiment. That is to say that the anthropologist biased the test results in favor of his own findings. This danger is obvious when the anthropologist does his own interpretation of the tests, but it is equally present when someone else interprets the results for the anthropologist. This is because the anthropologist has always consulted with his interpreting psychologist, and there are very few cases in which the anthropologist did not tell the psychologist much about the culture before, during and after the process of interpretation of the test results. It has seemed obvious that this must be done in order that meaningless interpretations of the Rorschach test, for example, might be avoided. In this way, however, we come face to face with the principle of *indeterminateness* in the observation-test problem; for in order to make the "objective" instrument work in the situation and produce meaningful results, we rob it of its objectivity.

Another factor that enters the picture at this point is the cultural bias of the instrument *and* the observer together. One of the most striking findings of projective testing of primitive people, and in general of people from cultures other than our own, is that they regularly turn out to show signs of deep-going psychopathology; and the anthropological works on projective tests are full of the language of psychopathology. It is difficult to understand why this should be so, unless we take the position that everybody is sick but the observer. Our impression is that not only do such interpretations stem from preconceived notions of what is "normal," but that something inherent in the instrument's past history in our culture compels us to see the responses of peoples from other cultures as psychopathological. What this something is, is clearest in the Rorschach test, where preponderance of small detail (Dd) responses, poorly assembled wholes, certain proportions of human movement (M) to color (C) responses and so on, are considered evidences of psychopathology. The traditional anthropological position in these matters still seems to

us a good one: psychic characteristics are to be thought of in terms of the individual who has them and in terms of the response of the environment to those characteristics. A person may have what we might call "psychotic" or "neurotic" characteristics, but if the environment does not become punitive or does not withdraw, the individual may go on functioning without serious difficulty. The crux of the matter is the response of the environment to the psychic traits of the individual. (This matter has been brought home to J. Henry and made particularly vivid through his discussion of the problem with Dr. Humphry Osmund, Director of the Psychiatric Hospital, Weyburn, Saskatchewan.) It seems to us impossible, therefore, to engage in cross-cultural diagnosis via projective tests, or in any other way, unless the response of the environment to the individual's psychic characteristics is known. After all, this turns out to be just good clinical psychology, for clinical psychology developed as a diagnostic device for the purpose of special study of people rejected by or unable to get along in their human environment.

To summarize this section, psychological studies in anthropology make their contribution, side by side with the more traditional branches of anthropology, to a global understanding of man and his evolution. Direct observation of the human organism functioning in its native habitat is an important device for reaching this understanding, when it is carried out systematically over periods of considerable duration, and manipulated quantitatively. It then provides data on the conditioning and learning process, which generally is not provided by the cross-sectional psychological tests. It may also pick up leads to the understanding of behavior characteristics which might have been missed had the whole observational situation been confined to tests alone.

Meanwhile it is important to note that psychological tests in anthropological field studies may not at present be considered to be more precise or comprehensive than direct observation. Furthermore, because of the principle of indeterminateness, psychological tests may not be considered as a check on direct observation, yet they have value in field studies by expanding the scope through increasing the range of sampling, and through turning up facts and giving insights that direct observation alone might not reveal. All of this, however, is contingent on observing certain precautions to be discussed in the next section.

II

Henry and Spiro (1953) have pointed out the shift of orientation from anthropological field studies concerned with groups to the study of the individual. They suggested that interest of anthropologists in cultural dynamics and cultural integration was significant in this shift of emphasis, particularly in personality and culture studies—the "cultural anthropology" of American and "social anthropology" of Britain. Paralleling this shift in anthropological

field studies is a similar reorientation in clinical psychology, extending presently not only to gathering data on human behavior but also including statistical treatment of "individual-centered" psychodiagnostic studies. This latter trend has been reviewed by Mensh (1950) in a survey of statistical techniques in present-day psychodiagnostics. It would seem then that there has occurred the psychological threshold in at least cultural anthropology and clinical psychology, marking a new awareness of the importance of the individual in studies of human behavior.

The use of tests grew out of study of individual differences (Boring 1929; Freeman 1939), originating with Galton in England, but influenced principally in the early developmental years by Binet in France. Just as "the study of personality and culture, with notably few exceptions, has been exclusively an American concern," so the movement in psychological testing also has been essentially American (Boring 1929; Henry and Spiro 1953), though this is not to say that European contributions to the development of testing have not been important. Boring described Galton as "a genius . . . ."

> His scientific contributions, beside his early ventures in exploration, include investigations in simple mechanics and the invention of apparatus, his persistent interest in meteorology, his continued study of inheritance and his foundation of the art of eugenics, his development of Quetelet's methods of statistics and their application to anthropological and psychological problems, his numerous researches in anthropometry, and his initiation of the experimental psychology of tests in England. (p. 454)
>
> . . . To measure the capacities of a large number of persons, and thus to sample the populations, requires as a practical matter the development of apparatus and methods by which the measurements of a single individual can be easily and quickly made. . . . For this purpose Galton invented the 'test,' and in particular the 'mental test,' an experimental method of measurement which is characterized by its brevity. . . . (p. 474)

Historically, it was J. Mck. Cattell in America who first used the term "mental tests" in 1890, and influenced the American tradition of individual psychology and mental tests. Galton's *Inquiries into Human Faculty and its Development* (1883) was followed by Kraepelin's and Oehrn's development of tests for research in psychopathology in the 1890's, Ebbinghaus' invention of tests in Germany in the same decade and Cattell's introduction of the term mental tests (1890: 15, 373–380), and the research in France of Binet and Henri. By the turn of the century the test movement included (Boring 1929) in addition to Cattell, such early psychologists in America as Jastrow, Boas, Münsterberg, Gilbert, Farrand, Sharp (a student of Titchener), Thorndike, Woodworth, and Helen Thompson, and the anthropologist Wissler.

The major shift from individual to group tests came with World War I and in the United States over 1,700,000 men were given the Army Alpha Test. During the 1920's the awareness of the value of tests in education greatly in-

creased their use. For example, a single test-publishing firm distributed 2½ million copies of intelligence tests in 1922–23 (Freeman 1939), and this increased use of tests has continued in educational systems at all levels of schooling. World War II produced another great demand for psychological measures, and during the war period some 16 million individuals had responded to some form of psychological test. Since the recent war, however, increased attention has been paid to individual studies rather than to group studies, and the controversy of nomothetic versus idiographic orientation (Mensh 1950) has become prominent again, in part a function of the increased emphasis on projective techniques as psychological test methods. The use of such techniques recently was reviewed by Henry and Spiro (1953) at the Wenner-Gren Foundation International Symposium on Anthropology.

The use of psychological tests, whether projective devices or more structured techniques, demands that certain criteria be observed. It is just here that the crucial problem of psychological tests in anthropological field studies lies. Uncritical use of tests introduces many uncontrolled biases and errors in the collection and analysis of data. Fortunately, anthropologists have recognized the limitations placed on psychological test data when criterion conditions are not met. Thus, more than a decade ago Hallowell (1940, 1941) cautioned that adequate sampling was necessary in the study of nonliterate societies, and that data must be collected for local group norms. Du Bois (1944) also reported "two cautions"—the lack of quantification and the great range of experiences sampled. And Henry (1941) reviewed "common experiences in Western culture" which are not common to other societies, including the "test-taking attitude," familiarity with pictorial representations of objects, and language and other symbolism differing from that of Western culture experience.

The "common experiences" noted by Henry form the basis of the rationale of Cattell's test (1940; Cattell *et al.* 1941), and Cattell concluded that "suitable tests could be built on a properly investigated 'greatest common knowledge.' " These common experiences were seen as the human body in its various parts, processes, and systems; and natural, environmental elements as smoke, fire and water. Cattell has utilized spatial perception as a more universally common experience than verbal, symbolic, or mechanical skills. He emphasized, as has Klineberg (1927), the significance of motivation, and "habits of attention and normal speeds of working" in cross-cultural studies. Abel and Calabresi vividly portray the problem of motivation:

> The children did not enjoy being the center of attention in the testing situation. They were shy, ill at ease, and unaccustomed to taking tests involving much talking, and they soon became weary with the effort of expressing themselves. There is a general feeling in the village that it is not good for a person to study or think too much, and the questions were *molestia* (a bother). One mother objected to the continuation of a test

on the grounds that . . . the child's head would get too hot from thinking so much! (Lewis 1951: 307–308)

Such cultural attitudes as these stimulate the question of how representative of the children's behavior are the data thus obtained. They may represent accurately inter-culture interaction of observer and cultural representative, but what do they tell us about intra-cultural behavior? Further, several anthropological field studies have gathered psychological test data at one period and brought them to the psychologist for scoring and interpretation at a later time, as long as six years in some studies. The extent of observation at time of administration of the tests may significantly affect the meaning of the test scoring and interpretation, because the contextual data of the subject's behavior in the test situation are extremely important in a clinical study. This is the well-known problem in the comparison of psychometrician, test-bound to the cook-book method, and clinical psychologist who utilizes the total behavior of the subject in the test situation to understand personality, and not merely the test scores.

What then are the criteria which the psychologist insists upon if psychological tests are used in anthropological field studies? The major contribution of the test is its objectivity and standardized procedure in administration, scoring, and interpretation. This objectivity and standardization is a function of several factors; and criteria for the selection of a mental test for anthropological field study are neither less important nor different from the criteria observed in other investigations of human behavior whether by psychologist, psychiatrist, sociologist, or other social scientist. Some of the significant factors (Freeman 1939; Watson 1951) in choice of a psychological test for a specific research design are: economy of subject and examiner time in administration, and examiner time in scoring and interpretation; cost; completeness and convenience of material; fullness, simplicity and clearness of directions; adaptability to subjects under investigation; appeal value; length of test [" . . . chance errors are diminished by an increase in the number of responses. . . . (but) What is the point at which we begin to have rapidly diminishing returns from an increase in the length of the test?" (Freeman 1939)]; ease and simplicity of administration and scoring, and of the response required of the subject; availability of suitable norms and their reliability and validity for the specific research; and relevance of test data to the problem under investigation. Finally, there are the significant conclusions of Henry (1947) which should be considered.

The single most outstanding limitation of a study of this sort is the unfortunate fact that the method of analysis for individual and for group use is so extremely difficult to document and delineate for other investigators. This fact is partly due to the newness of the techniques and the scarcity of research reports on the method of analysis.

It is to a very large extent due, however, to the fact that much of the methods and principles of analysis must lie within the training and experience of the analyst in general psychological theory and practice rather than within his mere acquaintance with the material of the TAT alone. (p. 127)

These then are criteria formulated by the psychologist, but there also are certain conditions presented by the anthropologist. Henry and Spiro (1953) list four requirements: measurement of the personality "as a whole" rather than by limited techniques, material which is not "culture-bound," tests permitting sampling of large numbers of subjects, and brief tests capable of rapid and easy administration, the analysis of which may be done by other than the investigating anthropologist to permit "the checking of the data by independent analysts and by persons whose judgment was not biased by previous knowledge of the people."

In cooperative enterprise both the anthropologist and the psychologist can meet the criteria which each desires of psychological measurement. Not every requirement can be met, however, and the investigator must attempt a balance between flexibility and objectivity which does not sacrifice one or the other quality. For example, although Cattell (1940; Cattell *et al.* 1941) has designed what he terms a "culture-free" test, Vernon (1951) has written that it is " . . . virtually impossible to find a culturally neutral test for the purpose of making interracial comparisons," and De Groot (1951) has made a similar statement on the basis of his experience. Further, in a recent review by Auld (1952) of the influence of social class on personality test responses the conclusion was drawn that error is introduced unless the investigator "consciously take(s) social-class differences into account." If one extrapolates from the differences between middle- and lower-class responses in our culture to the differences between our culture and others, the influence of cultural differences raises a significant problem in the use of normative data obtained in our society but used as reference in other cultures.

Other sampling and normative difficulties are reflected in such studies as that of MacGregor (1946) in which Indians of mixed heritage were studied, raising the question of appropriateness of norms—whether to use data from Indian or from white samples as norms. Also, how responses are affected by change in the stimuli demands an entire study in itself. In MacGregor's study, the TAT stimulus cards were "redrawn to present Indian characteristics," yet analysis and interpretation of the data were carried on in terms of norms derived from a different set of stimulus cards presented to a sample culturally different from the experimental group under study. Similarly, in Vogt's study of Navaho veterans (1951), a set of stimulus cards was devised which pictured the veteran in "both white and Navaho social situations." However, the standard series of TAT also were used, and in such a research design where both the standard set and an experimental set are used, there are then avail-

able referent data from the responses to the standard cards. Lantz's review (1948) of the studies of Cook, Hallowell, and DuBois also stresses the normative problem:

> The procedure of using Western norms when they corroborate the ethnographers' results, and of using explanations from the immediate culture when Western norms fail, seems highly unreliable and invalid. . . . In general it appears that the findings of Rorschach studies of pre-literate societies tend primarily to be highly general and somewhat vague . . . in part due to the inexperience of those using Rorschach as a test for cross-cultural personality investigation. Rorschach . . . devised . . . to describe the dynamic intellectual-emotional configuration among persons in Western society. Using it otherwise results in much speculation without any real scientific basis. (p. 291)

Other sources of error may arise in the interpretation of test data, even though administration of the psychological test was conducted with standard material. An example of this sort is seen in Mead's report (1949) on the Mountain Arapesh where Dr. Mead describes the several ways in which Unabelin's Rorschach data were analyzed four times by five clinical psychologists. Two of the four analyses are based on the protocol and localization chart; the other two also used the scoring, done by a sixth person. Dr. Klopfer discussed his interpretation with Dr. Mead, "occasionally asking . . . corroborative questions about the culture." Drs. Harrower and Miale discussed the protocol and chart, and "summarized the discussion. . . . followed by a long interpretative discussion among the three of us (including Dr. Mead), from which special points have been selected for publication." Dr. Wolfenstein "made a very rapid assessment . . . and Dr. Abel . . . wrote an analysis emphasizing the cultural relevance of the material." It seems apparent that the same criteria were not applied in the four analyses, nor was the same amount of data used. Further, independent interpretations were made in some instances and not in others, and knowledge of cultural factors was available to some of the psychologists and not to others. This variation in task and material and independence of report does not suggest a rigorous test of the use of the Rorschach technique in studying personality. Dr. Mead mentions "the variety of types of interpretation that have been used on this single (Unabelin's) Rorschach record," and there is indeed a variety, but then there is such a variety of conditions under which the five psychologists worked. Mead also reports "a high degree of agreement," but this does not seem to be supported by the objective evidence of the emphasis by one interpreter on the psychotic elements, because of unawareness of cultural factors; in another case, emphasis on timidity, fear, and caution; intellectual aspects emphasized in a third interpretation; and concentration on specific cultural factors as aids to interpretation in a fourth analysis. Whether these differences are a function of the interpreter, the Rorschach method, the amount of data, or the task

assigned each psychologist, cannot be said in light of the varying conditions obtaining in at least the latter two factors.

Sampling variation and its effect on representativeness of sampling has been carefully considered by Du Bois in connection with autobiographical material, and her concern applies equally well to sampling when psychological tests are used.

> The persons from whom autobiographies were secured do not represent the ideal or "type" person. . . . The most successful men said they were too busy. . . . The "ideal" women . . . were either too unassertive or too engrossed in work. I am under the impression that inability to secure autobiographical material from the successful type individuals of a culture is an experience many ethnographers have had in functioning societies. (Du Bois 1944: 191)

In the same study, Kardiner pointed to other factors capable of biasing data. First, there is the subjects' reaction to the ethnographer, seen for example in their seeking rewards for responding and in the seeking out of the ethnographer by some subjects who wanted the opportunity of being interviewed. Second, the presence of an interpreter has an effect which has not been controlled experimentally, although anthropologists frequently have discussed interpreter influence in studies. Further, the criteria of standard administration of psychological tests were not met, as seen in Porteus' comments on use of the Porteus maze test.

> . . . at a great disadvantage in not being able to observe the test responses . . . whether the mistakes were due to impulsiveness or to mental confusion and anxiety. . . . There were quite a number of cases in which there was evident misunderstanding of the nature of the test . . . native peoples . . . are less willing to attempt the test and seem to think that it represents a task that is outside the feminine province . . . your (Du Bois') results are not perhaps strictly comparable to mine, as the conditions of testing were somewhat different. Your subjects had the advantage of an interpreter and you also modified the procedure by marking the point of exit in each test. (Du Bois 1944: 553–554)

One more comment seems in order. It has been noted that "modal" or "basic" personalities have been constructed out of the statistics derived from psychological test data. This is seen, for example, in the description of the Alorese (Du Bois 1944). Statistics are short-hand methods for communicating certain information about characteristics of the distribution of data, and therefore valuable in scientific studies. However, a mode or mean statistic of central tendency tells nothing of the extent of variability of the data from which the central tendency is extracted. The standard deviation and form of distribution, whether normal, J-curve, or bi- or multi-modal also must be considered in reporting data. And it is the rule rather than the exception that field studies using psychological tests have revealed extreme variability among

the samples studied, so that a mean or modal value reported for a culture often is misleading in that it neglects to record the more significant factor of wide variation. Thus, to say of a sample of a culture that the mean number of Dd responses is 9 has significantly different meaning with a standard deviation of 10 than in a more homogeneous sample with much less variability (Du Bois 1944).

These several samples of possible biases in the administration, scoring and interpretation of psychological tests in anthropological field studies are not intended as condemnations of the studies or techniques. Rather, they warn of the care necessary in the use of tests, and also illustrate the awareness of anthropologists and psychologists of the problems of psychological measurement. Henry and Spiro (1953), Cook (1942), Hallowell (1940; 1941), Du Bois (1944), Henry (1941), Joseph and Murray (1951), have recorded the errors in sampling, normative data, and test technique which limit the results of their studies. Psychologists also have been concerned with the uncritical use of psychological tests, reflected, for example, in the reviews of Lantz (1948), McNemar (1952), Auld (1952), and Heathers (1952). The solution to the problem of adjusting the test criteria of the psychologist to the field study criteria of the anthropologist does not lie in the abandonment of tests, but rather it seems to be in the more critical use of tests and test data. The difficulties raised in the field by use of interpreters, modifications of test material and procedure, cultural attitudes, sampling variation and lack of adequate norms; and the difficulties in the psychological laboratory to which field test data are brought, because of the necessity for "blind analysis," are factors which limit the generalizations from field studies but do not demand that the data be discarded as useless. Instead, both the anthropologist and psychologist must see to it that their criteria of test selection and procedure are observed and limiting factors clearly stated. Further, there is to be considered the degree of "blindness" of the psychologist in blind analysis—how much information about the subjects and their culture can be revealed without biasing the data interpretation. For example, Watson (1951) has summarized his philosophy of the clinical method in the following manner:

> Regardless of the nature of the clinical problem, it is unlikely that any one battery of tests will permit the derivation of a complete diagnostic appraisal . . . valid interpretation . . . demands complementation from . . . developmental and school history, mental status examination, . . . entire case study . . . (these) are needed for adequate interpretation.

In summary, psychological measurement had its beginning in part in anthropological studies; it has moved from individual- to group- and now back again to individual-oriented emphasis in both anthropology and psychology; and it has been valuable to both fields in quantifying information

gathered under objective and standardized conditions. There are criteria of test selection and procedure which must be satisfied, and other criteria of the cultural anthropologist which can be met in application of tests in field studies. Limiting factors in terms of tests or field conditions do not demand abandonment of the techniques but do require scientific caution in the generalizations drawn from the data. Anthropologists fortunately are aware of the limiting conditions of language barriers, differing cultural attitudes and experiences, and inadequate normative data, but often there is too much emphasis upon psychological tests and other observational data without the reminder that different test conditions, from administration through the normative comparison stage, introduce distortion and bias which is uncontrolled so that evaluation of the data is difficult.

Although both anthropologists and psychologists have formulated criteria for psychological measurement in anthropological field studies, there remain several problems on which both anthropologists and psychologists can work collaboratively. The accumulation of adequate norms and discussion of "culture-free" qualities of psychological tests, and the limits of "blind analysis" of field data in the laboratory represent major areas where community of interests and experiences may be productive.

In applying psychological measures to the data of anthropological field study, the criteria of selection and administration of the techniques, and scoring and interpretation of the responses, constitute the scientific basis for the utility of the measures. Validity; reliability of the test situation and of the responses to it; ease and economy of administration, scoring and interpreting; applicability and relevance to the goals of the specific study—all these characteristics are as significant to anthropology's use of psychological techniques as to other applications. Multidimensional research has many advantages, but uncritical use of techniques from the various cooperating sciences, principally a function of the lack of communication among the disciplines, may seriously reduce the value of the "team approach." The objectivity and standardization of the psychological test are its major strengths, and if these are sacrificed then the test data offer no more than do other avenues for observing human behavior. The psychological test as a scientific experiment is valuable only when it is conducted with scientific control, in its elements of selection and administration, and scoring and interpretation of its data. A survey of the literature on anthropological field studies which have included psychological test measurement suggests that greater attention must be paid to the criteria for adequate psychological evaluation. Psychological measurement has demonstrated its significance in many areas of human behavior study but there are cautions to be observed and the specialized nature of tests requires trained personnel and an appreciation of the limitations of tests as field conditions vary.

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THIS ISSUE, AND OTHERS

This is the first issue which follows another by only two months. We are fully on our 6-a-year schedule, and ought to have a lot of room. But this issue is embarrassingly full. This (new) department was intended to face page 461, but has to be squeezed instead into odd half-pages, like other ads.

These paragraphs are not a substitute for an editorial. We have nothing to preach about; when we do, an editorial will appear. The response to previous editorials is such that we are deluged with good material, and we may soon face the need to enlarge the journal again. Eventually might anthropologists require a weekly to keep pace with new knowledge?

The Bartholomew-Birdsell article beginning on the page opposite is a fruit of inter-societal cooperation as explained in the footnote. The authors, incidentally, are both in Australia, presumably continuing their teamwork (Bartholomew is a zoologist). In the next issue a second article obtained through AJPA help will appear—showing the racial distribution of the sickle cell (blood, not soviet prison) mainly in Africa, with its implications for the anthropology of the new world Negro.

Interdisciplinary work is also reflected in the lead article in this issue. Ivan N. Mensh is in the department of neuropsychiatry at Washington University. Jules Henry, incidentally, has leave this year from Washington University for a special assignment at the University of Chicago, where his advice is con-

(*Continued on page 514*)

Ecology and the Protohominids*

By GEORGE A. BARTHOLOMEW, Jr. and JOSEPH B. BIRDSELL

ALTHOUGH the word ecology is used in both the biological and the social sciences, attempts to bring the biologist and students of human society together by analogical reasoning are beset with traps for the unwary. The biological world lies primarily within genetic and physiological limits while that of the social sciences lies within cultural limits. However, whatever else man is, he is first an animal and hence subject, although usually indirectly, to environmental and biological factors.

It is generally agreed that the ecological generalizations and points of view which have proved helpful in interpreting the natural history of most mammals can be applied virtually intact to all primates except man. It should, therefore, be possible to extrapolate upward from ecological data on other mammals and suggest the biological attributes of the protohominids and to extrapolate downward from ethnological data on hunting and collecting peoples and suggest the minimal cultural attributes of the protohominids.

We propose first to discuss in general terms some aspects of mammalian ecology which appear to be applicable to the protohominids; second, to apply these ideas to the available data on the australopithecines; and third, to discuss the application of a few ecological ideas to preagricultural humans. A history of the development of ecology and suggestions for its applications to anthropology which has recently been published by Bates (1953) provides basic historical orientation and perspective for such an effort.

Protohominids and tools. In retrospect, the vast sweep of evolution appears to lead inevitably to the appearance of man, but a rational interpretation of the evidence refutes this. During the Cenozoic there have been three separate mammalian evolutionary complexes, one in Australia, one in South America, and one in Eurasia, Africa, and North America. Of these complexes, only the last has produced organisms of the hominid level. Further, since the major orders of mammals were already distinct in the Eocene, each has had a separate genetic history for approximately 70,000,000 years, and only one, primates, has produced an organism at the hominid level of organization.

Since a number of mammalian orders have shown a strong independent evolutionary trend toward a large brain size, this trend is by no means peculiar to the order primates (Edinger 1948). This striking parallelism is presumably related to the fact that large brain size favors varied behavior and learning as supplements to genetically fixed responses. Why then did not the primates,

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like the other mammals, reached an apparent evolutionary dead end in the Pliocene? The familiar and reasonable ideas concerning the importance of arboreal life in setting the stage for the appearance of man, i.e., dependence on vision, grasping hands, and the lack of restrictive skeletal adaptations, need not be labored here, but the importance of bipedalism can profitably be re-examined.

The primates comprise the only major order of mammals which is characteristically arboreal. There can be no doubt that this arboreal heritage has been of vital importance in human evolution, but the critical stage in the transition from ape to protohominid involves the assumption of a unique terrestrial mode of life. A number of cercopithecids have successfully invaded the terrestrial habitat, but these all show quadrupedal adaptations. This level of adaptation, while obviously effective if one may judge by the fossil record and by present abundance, appears to represent a stable, long-surviving, adaptive equilibrium.

The terrestrial adaptations of the hominid line represent a step into a new and previously unexploited mode of life in which the critical feature was bipedalism. Among mammals changes of this magnitude have occurred only rarely since the middle Cenozoic. Aside from the saltatorial rodents such as the jerboas and kangaroo rats, all placental terrestrial mammals other than man use both hind and front legs for locomotion. The extreme rarity of bipedalism among mammals suggests that it is inefficient except under very special circumstances (Hatt 1932; Bartholomew and Caswell 1951). Even modern man's unique vertical bipedal locomotion, when compared to that of quadrupedal mammals, is relatively ineffective, and this implies that a significant nonlocomotor advantage must have resulted from even the partial freeing of the forelimbs. This advantage was the use of the hands for efficient manipulation of adventitious tools such as rocks, sticks, or bones. Of course, the terrestrial or semi-terrestrial living primates have their hands free when they are not moving, but only man has his locomotion essentially unimpeded while carrying or using a tool. Man has been characterized as the "tool-using animal," but this implies a degree of uniqueness to man's use of tools which is unrealistic. Not only do other primates use tools—the use of sticks and rocks by chimpanzees and baboons is generally familiar—but such unlikely animals as the sea otter (Fisher 1939) and one of the Galapagos finches (Lack 1945) routinely use rocks or sticks to obtain food. Indeed, the natural history literature is replete with instances of the use of tools by animals, and there really is no clear-cut boundary between web-spinning, nest-building, and stick-wielding on the one hand, and tool use at the simplest human level on the other. However, in contrast to all other mammals, the larger arboreal primates are, in a sense, tool users in their locomotion. As they move through the maze of the tree tops, their use of branches anticipates the use of tools in that they

routinely employ levers and angular momentum. The grasping hands on which the locomotion and feeding of primates depends, are of course obviously preadapted for tool use.

Rather than to say that man is unique in being the "tool-using" animal, it is more accurate to say that man is the only mammal which is continuously dependent on tools for survival. This dependence on the learned use of tools indicates a movement into a previously unexploited dimension of behavior, and this movement accompanied the advent of bipedalism. With the assumption of erect posture regular use of tools became obligatory; the ability occasionally to use tools must have preceded this in time.

Protohominids and body size. The conditions of terrestrial life for a bipedal tool-using mammal virtually demanded that the protohominids be big mammals, i.e., at least in the 50 to 100 pound range, for large size of itself offers important biological advantages (Carter 1951: 293). In the case of the protohominids two such advantages at once suggest themselves: First, large size would remove them from the category of potential prey for all carnivorous birds, reptiles, and all mammals except the big cats and the pack-hunting dogs; second, it would allow them to utilize without restrictive anatomical specialization and with simple instrumentation, virtually the entire range of food size utilized by all other terrestrial mammals.

Sociality. Social behavior is inextricably interwoven with ecology, and although it is not possible to review the subject in detail here, certain aspects of it are basic to the development of later ideas.

The transitional protohominids must have been social to the extent of forming relatively stable family groups. Even in the absence of direct evidence, such a statement can be made with complete confidence from knowledge of the other members of the suborder Anthropoidea. First, there is the absence of seasonal sexual periodism in man and the great apes. Thus sexual ties form a bond of sustained and continuing attraction which provides a biological basis for the long-surviving family unit. As has frequently been pointed out this is a central element in human sociality. Second, there is a long period of growth and maturation. The long childhood of man and the great apes is not a mere function of size—the blue whale, the largest mammal that has ever lived, grows to sexual maturity and to a length of 70 or more feet in two years (Mackintosh and Wheeler 1929)—but it is related to the unique dependence for survival on learning in the higher primates. The acquisition of competence for independent life demands several years of parental care in the chimpanzee and a decade or more in man. Hence, survival requires a mother-offspring relation which is sustained through many years and, like sexual attraction, is not just a seasonal interlude as in other social mammals. Since these factors shape the social behavior of both the great apes and man, they must have shaped the social life of the protohominids.

Other cohesive forces, by analogy with living primates, must have supplied integration to the social organization of the protohominids. Important among these must have been dominance-subordinance relationships. The concept of social dominance has proved to be a touchstone to the understanding of the social behavior of vertebrates. It is a key factor in the social behavior of mammals as diverse as deer (Darling 1937), seals (Bartholomew 1952), and primates (Yerkes 1939).

In every case in which it has been studied in mammals, dominance is established at least in part on the basis of aggressive behavior (Collias 1944), of which a large component is either directly or indirectly dependent on reproductive physiology. In mammals the male sex hormones stimulate aggressive behavior and contribute to greater body size, while the female sex hormones inhibit the former and do not contribute to the latter. Consequently, males tend to be dominant over females in most situations. In the higher primates, as in many other social mammals, sexual dimorphism in size reinforces the greater aggressiveness of the male and insures his superior social status in situations where force is involved. In most social mammals, gregariousness overcomes the disruptive effect of dominance-subordinance relations and maintains the social unit. In primates dominance is not an exclusively disruptive force, since the dominant animal may protect the subordinate animal which looks to it for protection as well as leadership (Noble 1939).

In nonprimate social mammals, the resolution of the forces produced by dominance and gregariousness typically produces a seasonal breeding unit which consists of a dominant male and a harem of females and which usually excludes the young of previous years.

The social unit in nonhuman primates is variable, and too few detailed field studies have been published to allow extrapolation from living anthropoids to the protohominids. In modern hunting and collecting groups of man the smallest unit is the biological family including immature offspring, and in many cultures the most important functional group is the extended family, or band. In the case of man, even at the simplest level, social dominance is not based exclusively on successful aggressive behavior. The distance between nonprimate mammals and man is too broad to be spanned by the bracketing technique previously used, but the semi-permanent biological family, including offspring, must have been a basic unit among the protohominids. Integration on any more extensive scale must have depended upon the degree of cultural attainment. It should be observed however, that fairly large groups have been reported for living nonhominid anthropoids (Carpenter 1942; Nissen 1951).

Territoriality. No aspect of the social behavior of wild vertebrates has attracted more attention than territoriality, a concept which includes the entire complex pattern of behavior associated with the defense of an area. The display of ownership of places and objects is very highly developed among human

beings, but this behavior pattern is not peculiar to modern man. It is almost universally present in terrestrial vertebrates, either on a permanent or seasonal basis. The large literature on the subject with regard to birds has been reviewed by Nice (1941). Its status in mammals has been discussed by Burt (1943), and its relation to vertebrate populations has been examined by Errington (1946).

Territoriality springs from the necessity for finding and maintaining environmental conditions suitable for survival and reproduction. The techniques of territory maintenance, the precise factors immediately responsible for it, and the immediate significance of it vary from species to species.

The maintenance of territories either by individuals or by social groups has profound effects on distribution. Birds and mammals tend to be neither continuously distributed nor irregularly grouped, but to be spaced at more or less regular intervals through ecologically suitable habitat. This spacing is determined by conflicts between pairs of individuals or between interacting groups of animals. Thus, territorial boundaries are learned and vary in time and space. If anthropologists were willing, this might almost be considered protocultural behavior at a subhuman level; in any event, it emphasizes the continuity of human behavior with that of other vertebrates.

As a result of the centrifugal effects of aggressive behavior, territory maintenance forces animals to disperse into adjacent areas. It distributes the individual organisms or social units of a species throughout the entire accessible area of suitable habitat. Should the population increase, local population density does not continue to build up indefinitely. Instead territorial defense forces individuals out into marginal situations, and thus the resources of the optimal habitat are not exhausted. Most of the displaced individuals do not survive, but some may find unexploited areas of suitable habitat and thus extend the range of the species. The result is that a population tends to be maintained at or below the optimum density in the preferred habitat, and the excess individuals are forced to marginal areas to which they must adapt or die.

Thus territoriality is one of the primary factors which determine the density of population. It organizes a local population into a well-spaced array that allows adequate living conditions for all successful individuals. It limits the breeding population which can exist in suitable habitats and thus helps to prevent increase beyond the long-term carrying capacity of the range. This dispersive effect of territoriality can hardly help but be an important causal factor both in migration and in the spread of genes through a population. Hence, it must contribute importantly to rate of evolutionary change (Burt 1949).

The question of the importance of territoriality to the biology of protohominids at once presents itself. Carpenter (1934; 1940) has demonstrated that howler monkeys and gibbons maintain territory by group action. It is

clear that territoriality exists in all complex human societies, and it is clearly established that group territoriality is also important at the simplest levels of human culture. It is, therefore, reasonable to assume that protohominids similarly possessed a well-developed territoriality, presumably on the basis of the family or extended family.

Population equilibrium. One of the most critical ecological factors which can be determined about an animal is the density of its population. The number of variables which contribute to the determination of population density is enormous; a complete analysis for even the best known of living wild mammals is difficult, perhaps impossible. Nevertheless, the framework within which such an analysis can be made is known, for the factors involved in population dynamics have been studied intensively in recent years. A useful discussion of populations from the point of view of the ecologist is given by Bodenheimer (1938), and Allee *et al.* (1949).

Since organisms are transient biochemical systems which require continuous expenditure of energy for their maintenance, the struggle for existence becomes, in one sense at least, a struggle for the free energy available for doing physiological work. This fact offers a point of view from which to approach the problem of estimating the population of protohominids, or any other mammal.

There exists a series of nutrient or trophic levels that expresses the energy relations which tie together the various organisms of the terrestrial environment. The primary trophic level is that of the green plants, for only they can use radiant energy to synthesize significant quantities of organic material. The trophic level of the herbivores includes all animals directly dependent on plants for food. The next higher trophic level, that of the meat-eaters which may be primary carnivores (eaters of herbivores), secondary carnivores (eaters of other carnivores), and so on. The final trophic level, the eaters of dead organic material, eventually returns materials to the inorganic state depleted of biologically available energy.

Materials which are used as building blocks and sources of energy by organisms cycle continuously through these trophic levels, and at each level there is an endless competition for them. There are a number of obvious corollaries which follow from these relationships. An important one is that nutrition plays a primary role in determining the major functional adaptations of animals. Life demands a continuous expenditure of energy, and this energy is available only through nutrition. These energy relations involve a sustained long-term pressure sufficiently constant to maintain and give direction to the major evolutionary trends apparent in the adaptive changes of the sort shown by hoofed mammals and the carnivorous mammals. As Simpson (1944: 31) and others have pointed out, these nutritive adaptations have for the most part led not only to greater efficiency but also to more and more specializa-

tion, with a consequent reduction in potentiality for new major nutritive adaptations. Thus adaptations toward increased efficiency in food getting, or toward avoidance of becoming food for other organisms, are largely restrictive from the standpoint of future evolutionary change.

The total weight of biological materials produced by one trophic level must necessarily be less than that of the level below it on which it depends, and greater than that of the level above, which it supports. Each nutritive level must in the long run live on the interest, not the capital, of the trophic level below it. From this there follows a maxim which allows of no exception. On a long-term basis the mean population of a species is in equilibrium with the trophic levels both above it and below it, as well as with the total limiting effects of the inorganic environment. This means that the birth rate must be great enough to balance the death rate from disease (a nutritive phenomenon from the standpoint of the disease-causing organism), predation, and accident. Consequently, birth rate is a factor subject to natural selection, and all natural populations represent approximate equilibria between biotic potentials and total resistance of the biological and physical environments. Short-lived mammals of high fecundity, such as rabbits and mice, are sometimes characterized by drastic short-term fluctuations in population size, the causes for which are still subject to active controversy (Cole 1951). However, in this paper we shall ignore the problem of population cycles, for drastic cyclic fluctuations have rarely been observed in large tropical mammals with low reproductive potentials.

It has been generally appreciated since the time of Darwin that animals, despite their capacity to increase in numbers, tend to maintain a population which fluctuates around some equilibrium figure. This idea is of such a basic nature that it forms a foundation for the concept of natural selection which now appears to be an omnipresent evolutionary force. The factors involved in the maintenance of these equilibria are complex and variable. Since, as pointed out above, an animal population cannot possibly permanently exceed its food resources, these fix an upper limit. The determination of the actual equilibrium figure is a subtle problem which must be solved independently for each population. A thoughtful analysis of the factors limiting population in a non-hominid primate under natural conditions is presented by Collias and Southwick (1952) in their study of howling monkeys. For a population to maintain itself above that lower critical level which means inevitable extinction (Darling 1938), many factors (which may vary independently) must be simultaneously satisfied. Such things as a suitable habitat which will include adequate food resources, water, and home sites, and climatic conditions that do not exceed the tolerance of the group must be present.

Since biological factors vary with time, values for population equilibria are not to be measured at a given point in time. They fluctuate about a balance

which is determined, not by the mean condition, but by the extremes. Indeed, one of the most firmly established ecological generalizations is Liebig's law of the minimum, which states that a biological reaction at any level is controlled not by the factors which are present in excess, but by that essential factor which is present in minimal quantity. Since, as was previously pointed out, population density is the most critical single ecological datum, anthropologists studying the simpler cultures characterized by few storage techniques would do well to search for those critical limiting factors which do determine density. Such limiting factors are not necessarily either obvious or conspicuous at all points in time, and even when they occur their expression may be subtle or apparently indirect. A semi-arid area may have many fruitful years in succession, but a single drought year occurring once in a human generation may restrict the population to an otherwise inexplicably low density. For example, the Papago Indians of the lower Colorado River were forced in drought years to revert to a desert hunting and collecting economy for survival (Castetter and Bell 1942). Thus, their population density appears in part to have been strongly affected by the preagricultural carrying capacity of this area. In some cases the size of a population will be determined not by the availability of an abundance of food during ten months of the year but by a regular seasonal scarcity in the remaining two months.

The reproductive potential of animals is such that under favorable conditions, such as having available a previously unexploited habitat, the size of a population can increase at an essentially logarithmic rate. This capacity for rapid increase makes possible the recovery of populations following drastic population reduction. In a stable population, on the other hand, the reproductive potential is expressed only as a one-to-one replacement of adult individuals.

Anthropologists are properly impressed with the complexity of learned behavior in human groups, but may fail to appreciate its significance among other mammals. Even on the nonhuman level, population density may be controlled by behavioral factors, either genetic or learned. Territoriality and dominance relations, which are dependent on learned behavior, contribute to the determination of group relations and population density. Under certain circumstances behavioral factors may be more important than nutritive factors in determining population density. For example, recent work discussed by Calhoun (1952) has shown that the Norway rat under controlled experimental conditions, in which food is present in excess at all times, reaches a population equilibrium that is determined by strictly behavioral factors related to territoriality and competition for suitable homesites. Thus, experimental work confirms extensive field observations on a variety of vertebrates. Since learned behavior operates as an important factor determining density in all terrestrial mammals which have been studied, and in modern man, it

must have been an important factor in determining the population density of the protohominids. The importance of learned behavior increases directly with its complexity, and in man at cultural levels above the hunting and collecting stage of economy it becomes increasingly difficult to identify the ecological factors affecting population size.

ECOLOGY AND THE AUSTRALOPITHECINES

The dating of the australopithecines has proved troublesome, and final decision is not now possible. Dart and Broom have suggested that these protohominids lived during a period extending from the Villafranchian into the middle Pleistocene. This time span overlaps the datings of early man in other parts of the world, and implies a collateral relationship with more evolved hominids. Another view has recently been given by de Chardin (1952), who places the australopithecines in Villafranchian time, and thus removes them from contemporaneity with known African hominids. Breuil (1948) seems to reflect a similar point of view. In the former case the australopithecines would have been competing in their closing phase with more advanced forms of man, and hence would have been decreasing in numbers and range. In the latter instance the australopithecines would apparently have been the sole occupants of the protohominid niche over wide areas in South Africa, with the resultant possibility of having an expanding population and range. For purposes of an ecological discussion it is necessary to assume one dating or the other; it is not important to decide whether or not the australopithecines were in fact ancestral to more advanced hominid types, but it is important to determine whether or not they were the sole occupants of the hominid niche in South Africa.

As de Chardin (1952) points out, the australopithecine-bearing breccias and the human industry-bearing deposits have never been found conformably associated in the same site. This assumes, as did de Chardin, that *Telanthropus* is but a variant of the australopithecine type. Therefore, for purposes of discussion we shall assume that the autralopithecines are Villafranchian in date and hence earlier than the markers of the pebble-cultures of South Africa. By analogy with the ecology of other animals it would be surprising if man and the australopithecines had remained contemporaries in the same area over very long periods of time, for closely-related forms with similar requirements rarely occupy the same area simultaneously.

Use of tools. Neither the archeological nor morphological evidence concerning australopithecines suggests an alternative to the assumption, which we made earlier, that protohominids were dependent on the use of tools for survival. It is generally agreed that the australopithecines were bipedal. Referring to our previous discussion, this strongly implies that the australopithecines routinely utilized adventitious or perhaps even slightly modified tools. Dart's

evidence (1949) for the use of ungulate humeri as clubs offers empirical support for this theoretical position. Unmodified rocks used as tools can rarely be identified except by context. Familiar evidence from both archeology and ethnology shows that at the simplest level, rough tools commonly are discarded after initial use. Hence, a lack of recognizable stone tools in the breccias does not indicate that these were not used. Time alone precludes the survival of wooden implements such as clubs and digging sticks, although their use by australopithecines is certainly to be expected, for even the living great apes use sticks spontaneously.

The dentition of the partly carnivorous australopithecines (see section on food size) is uniformly characterized by reduced canines and incisors, and by nonsectorial premolars and molars (Le Gros Clark 1949). These dental characteristics are unique to them among all the large carnivorous mammals. The absence of teeth adapted for stabbing or shearing clearly implies the killing of game by weapons and butchering by simple tools. This observation would hold true even if the assignment of carnivorous habits to australopithecines were based only upon the abundant evidence that baboons were an important item in their diet. It is not dependent on the controversial question of their killing large hoofed mammals.

The dentition of australopithecines offers further evidence concerning their dependence on tools. As pointed out previously, intrasexual combat is characteristic of the males of virtually all strongly dimorphic mammals. Australopithecines are dimorphic, but they do not have the large piercing canines so characteristic of most of the larger living primates. This striking reduction of canines strongly implies that even in intrasexual (and intraspecific) combat, the australopithecines placed primary dependence on tools.

Scale of food size. It should be possible on theoretical grounds to fix the approximate upper and lower size limits of the food which could economically be handled by the australopithecines with nothing more elaborate than a crude stick for digging and a limb bone for a club. Their capabilities would allow the utilization of the following animal foods: virtually all terrestrial reptiles and the smaller aquatic ones; eggs and nesting birds; some fish; fresh-water mollusks and crustaceans; insects; all of the smaller mammals including some burrowing forms, and larger mammals up to and including baboons. It is difficult, perhaps impossible, to determine whether or not the remains of the large giraffids and bovids reported from the bone breccias (Dart 1948), represent kills by australopithecines or their scavenging from the kills of the larger cats. Since few meat eaters are loath to scavenge, and the implementation which would allow the australopithecines to kill such large animals is not apparent, we suggest that scavenging from the kills of the larger carnivores may have been systematically carried out.

Like most present-day hunting and collecting peoples, the australopithe-

cines probably used plants as their major source of food. Without imputing to the australopithecines any cultural capabilities beyond the use of a simple stick for digging, at least the following types of vegetable food would be available to them: berries, fruits, nuts, buds and shoots, shallow-growing roots and tubers, and fruiting bodies of fungi. Some of the very small vegetable foods exploited by modern human groups were probably not extensively used. Effective utilization of grass seeds and other hard-shelled small seeds require specialized gathering implements and containers, and processing by grinding or cooking.

Such activities imply technologies which cannot be assigned *a priori* to the australopithecines, and for which there is no archeological indication until much later times. In this connection it may be noted that the evidence for the use of fire by *Australopithecus prometheus*, though impressive, is still regarded by some as controversial (Barbour 1949; Broom 1950). In summary, it seems reasonable to treat the australopithecines as generalized carnivorous animals for which the freeing of hands and the use of simple implements enormously broadened the scale of food size to include a surprisingly large proportion of the total food resources of the terrestrial environment.

Social Behavior. The biological bases for the family and social organization at the protohominid level which have already been discussed should apply to the australopithecines. Group organization beyond the family level is not indicated by the archeological context of the finds, because the rather large number of individuals recorded from Swartkrans and Sterkfontein might result from sampling of family-sized groups over many generations. However, there is at least one line of archeological evidence which suggests social organization beyond the simple family level. Since baboons travel in large aggregations and were a significant item of australopithecine diet, it would seem likely that the latter hunted in bands. A single australopithecine, even armed with a club, would not be a serious threat to a band of baboons (Dart 1949). Such group hunting does not necessarily imply a high level of communication, such as speech, or permanence of organization, for it is characteristic of a number of nonprimate carnivorous vertebrates—many canids, some fish-eating birds, and killer-whales. Broom (1950) has shown that the australopithecines were characterized by sexual dimorphism, a widespread trait in the primates, including man. In social mammals, sexual dimorphism is almost invariably a product of the sexual selection associated with competition between males for females. Characteristically this sexual selection produces males which are larger, and more aggressive than females, and which have specialized structures for offense and defense. Although these dimorphic characters are a product of competition between males, they usually result in the males assuming the role of group defender. We propose that the sexual dimorphism of the australopithecine males may have favored a secondarily-

derived function related to aggressive behavior, namely the hunting of large prey, including perhaps other australopithecines (Dart 1949). Thus it may be that a sexual division of labor such as is present in all known hunting peoples was foreshadowed at this early level of hominid evolution.

The primates which first began to exploit a bipedal tool-using mode of life were establishing a level of adaptedness of enormous potentiality which had previously been inaccessible. They were entering a period of rapid change leading to a new kind of adaptedness. In the terminology of Simpson (1944) they were a group undergoing quantum evolution. It is to be expected that, like other similarly rapidly evolving groups, they would be represented in the fossil record not by a uniform long-persistent type, but by a variable group of related forms. The australopithecines, which probably occupy a stage near the end of a step in quantum evolution, fit this theoretical prescription nicely. The various australopithecine forms which have been named can be considered representatives of a highly polymorphic assemblage. Their polymorphism is consistent with the idea of a rapidly evolving and radiating group and thus favors the probability of the Villafranchian dating.

It is reasonable to assume that most of the recovered australopithecine fossils date from a period prior to the time they faced competition from more highly evolved hominid types. When, as they inevitably must have, the australopithecines came in contact with culturally advanced hominids, they must have been subject to rapid replacement in terms of geological time.

DISCUSSION

A paper such as this necessarily can be of only temporary utility. We feel that its principal contribution lies in raising questions, the answering of which may require orientation toward new points of view, the collection of new kinds of data, and perhaps the use of new techniques.

Students of animal ecology have developed a number of points of view which could be profitably applied to the study of preagricultural man. Two are particularly attractive. The first of these is that the basic problem of human behavior, like the behavior of other animals, is the obtaining of food, for the human body requires a continuous input of energy both for maintenance and for propagation. The second point of view involves the idea that population density normally is a complexly maintained equilibrium, dependent upon environmental as well as behavioral (and in the case of man, cultural) forces.

Anthropologists and archeologists to date have shown great ingenuity in utilizing the meager data for paleolithic and mesolithic man to establish tentative chronologies and outline cultural relationships. However, at the simplest level, the significance of material culture lies neither in the establishment of chronology nor as a measure of relationships, but as an indicator of

efficiency in obtaining food. The lack of data concerning the food-getting effectiveness of the various items of material culture primarily results from preoccupation with typology rather than function. Even the best of typological labels tend to restrict functional interpretations and to ignore the role of varied behavior and human ingenuity in extending an implement's utility. Furthermore, functional interpretations can be determined only by studies of living peoples, and the ethnologist has not yet generally been stimulated to the realization of the basic importance of such data.

It is of interest that some food-getting devices which we presume to have been available to the australopithecines remain important today in the economy of hunting and gathering peoples. But there is little systematic quantitative information concerning the proportion of food obtained through the use of the hands alone, or that added by the use of the simple digging stick, club, or wooden spear. Nor at a more culturally sophisticated level is there quantitative data available to measure the increase in efficiency made possible by the invention of such devices as the spear-thrower and the bow and arrow. In making such analyses it would be useful to distinguish between the contributions of the relatively limited variety of primary tools and the more varied secondary tools. For example, the ecological significance of the fist-axes of the lower and middle Pleistocene varies enormously depending upon whether they are to be interpreted as primary tools used to make wooden implements such as clubs, digging sticks and spears, or whether they are regarded in the unlikely light of hand-held striking implements (see, for example, Tindale 1949).

As pointed out previously, all animal populations, including human populations, depend on radiant energy stored chemically by photosynthesis. Animals compete endlessly between themselves for the one per cent of incident solar energy which plants are able to capture. The competitive success of an individual animal can be determined from its metabolism and the success of a population can be expressed quantitatively as the product of population density times individual metabolism.

If one can obtain even approximate figures for (1) the production of organic material by plants, (2) population densities and, (3) metabolism, one can evaluate from one point of view the biological success of different organisms. One can compare lions and elephants, earthworms and mice, humans and all other organisms, or more pertinent to anthropologists, one can compare simple cultures existing in either similar or different environmental situations. Since human beings comprise a single species, inter-group comparisons can be made on the basis of weight per unit area. An instructive analysis of this sort for small North American mammals has been made by Mohr (1947).

To our knowledge this quantitative approach to human ecology has not

been exploited by anthropologists; indeed, few attempts have been made by zoologists. Pearson (1948) has gathered figures which allow a comparison of Indians of northeastern United States with other animals common in the same area. Indians had less metabolic impact than deer, about the same impact as long-tailed shrews. Deevey (1951) presents calculations which show the amazing trophic impact of the present human population of the world. Both these efforts are frankly exploratory and depend on approximations, but they point up an approach which merits consideration by anthropologists. If one could obtain for given areas even crude figures for human population density and for the production of organic material by the flora, he could compare the nutritive efficiencies of rainforest and grassland cultures, or the efficiency of Great Basin Indians and Australian Aborigines even though the two peoples live in arid regions of a very different character. Similarly one could obtain quantitative estimates for the effects of rivers, lakes, sea shore, and particularly vegetation types (i.e., oak woodland) on the capacity of an area to support human populations at a simple cultural level.

As discussed earlier, natural populations tend to fluctuate about some equilibrium figure. This fact has long been recognized by biologists, but to date, despite the perspectives which it supplies, it has not significantly influenced the approach of most anthropologists. From a short-term point of view, populations are in only approximate equilibrium, but viewed from the time scale of the Pleistocene, slowly expanding populations of man can be considered as being essentially in equilibrium. It appears to us that the idea that the populations of early man were in approximate equilibrium with the environment can supply a point of view from which to interpret the dynamics of technologically simple human populations. It should greatly facilitate qualitative exploration of such considerations as spatial variation of population density; growth or decline in numbers; rates of movement as influenced by migration and gene flow; and, shifts of populations into new climatic situations which demand new modes of life and may involve biological as well as cultural changes in adaptedness. As such qualitative interpretations are refined it may be possible to develop models which depict these processes semi-quantitatively and thus allow crude predictions.

Population density is a key to these dynamic processes, for either directly or indirectly it controls all of the others. As discussed in the sections on territoriality and population equilibrium, the density of early human populations, while immediately determined by a complex of variables in which behavior plays a central role, was ultimately controlled by the environment. Even in the most favorable environments the equilibrium density attained by natural populations is somewhat below the maximum which the environment can support. The factors restricting density are behavioral in an im-

mediate sense and involve such things as aggressive behavior and territoriality. These behavioral factors must have brought dispersive forces to bear on Pleistocene man just as they do on other mammals. The existence of such dispersive forces suggests that the evolving australopithecines must have spread with great rapidity (i.e., almost instantaneously in terms of geological time) throughout the continental tropics and subtropics of the old world. Such an expansion would leave no suitable and accessible areas unoccupied. Consequently all subsequently evolved hominids in these regions must have expanded at the expense of already established populations. The replacement of the australopithecines by somewhat more advanced but related hominids may have followed the usual mammalian pattern of the gradual expansion of the more efficient form, and the slow reduction of the numbers of the less efficient. In many instances, however, population change must have resulted from gradual genetic penetration, and much of human evolution in the Pleistocene could easily have been powerfully affected by introgressive hybridization. In this regard it should be remembered that anatomical differences do not necessarily indicate genetic incompatability between groups, and that there is no evidence of reluctance to hybridize even between widely different human types. If rapid and dramatic group replacement did occur it must have been a rare event occurring in special circumstances.

Although mammals are less affected by climate in a direct physical sense than are most organisms, physiological differences among mammals adapted to different climatic conditions have been clearly demonstrated (Scholander, *et al.*, 1950). Distributionally the primates are an order characteristic of the tropics or subtropics. Modern man himself appears to be unable to invade the higher latitudes without fairly elaborate cultural accoutrements. It may therefore be concluded that during the Pliocene, the evolving protohominids occupied only the tropics, subtropics and perhaps the fringes of the temperate zones. The only place in which human populations could have expanded into a vacuum was at the margins of the then habitable areas. Thus changing cultural, and possibly changing biological, adaptedness would have allowed hominid expansion from the tropics into the temperate regions and ultimately into the arctic regions of the Old World. Aside from the initial continental expansion of the Old World protohominids, man expanded into major vacuums in populating Australasia, the New World, and much later, Micronesia and Polynesia. Once entered, these areas must have become filled rapidly, so that subsequent immigrants were faced for the most part with the problem of replacing established populations. Migrations, although spectacular, were probably of less importance in the Pleistocene than the processes discussed previously, which proceed normally without local catastrophic environmental change.

The anthropologists' lack of concern with the idea of population equilib-

rium in the simpler and more static human cultures is explicable in historical terms. Anthropologists, reacting to the claim by some anthropogeographers that extreme environmental determinism was operative on man, soon demonstrated that details of culture were not controlled directly by the environment. This broad denial overlooked man's nutritive dependence upon the environment, and long inhibited quantitative investigation of the relationship between man's population density and environmental factors.

The present interpretation of the mechanism of evolution is based upon natural selection which demands that populations be in a state of approximate equilibrium at a given time. To unravel the evolution of Pleistocene man, inevitably hampered as one is with inadequate data, one must necessarily use the idea of a population in equilibrium with the carrying capacity of the environment.

Most ecologists agree that no data are more crucial than those bearing upon population size, structure and density. Anthropologists, even though generally unconcerned with population equilibria, in some instances have been aware of the concept (Krzywicki 1934; Steward 1938; and Evans-Pritchard 1940). But in general the importance of an ecological approach has not been appreciated. Some archeologists have hoped to reconstruct preagricultural population figures from studying the temporal and spatial distribution of sites, but the inescapable sampling errors in this approach render it unreliable. We suggest that an analysis of the energy relationships and the efficiency of the techniques for obtaining food offer a promising approach.

For several years it has been apparent that an ecological approach is imperative for all studies in population genetics, including those pertaining to man. It also offers a potentially useful point of view to the physical anthropologist, the ethnologist, and the archeologist, and it should provide an important integrative bridge between the various fields of anthropology.

SUMMARY

An attempt is made to apply ecological concepts which are widely used by vertebrate zoologists to protohominids in general and to australopithecines in particular.

Various aspects of the biology of protohominids are considered: tool use, bipedalism, body size, scale of food size, sociality, territoriality, population density and equilibria, dispersion, and nutrition.

Energy relations to the environment and population equilibria are discussed with regard to human preagricultural populations.

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Some Aspects of Human Ecology in the Central Plains*

By WALDO R. WEDEL

THIS paper reviews briefly certain interactions between man and his physical environment in a selected section of the Interior Plains, an area of unusual interest from this standpoint. Generally favored with good soils and a long growing season, presenting no serious topographic hindrances to travel on foot or horseback in any direction, and supporting in its original grassland condition a great variety and abundance of wild game, this area has offered to man varied opportunities for development of subsistence economies based on food-collecting, food-producing, or both.

Since its settlement by white men scarcely a century ago, however, it has periodically presented major problems of adjustment—problems that were dramatically brought to the fore by devastating droughts and spectacular dust storms during the 1930's. It is abundantly clear that the climatic variations which characterize the region spatially from east to west also have their counterparts in fluctuations through time. In the perspective of archeology it appears that the native subsistence economies were also confronted with the need for re-adjustments from time to time.

For present purposes, the Central Plains may be defined as the region between the Missouri River and the Rocky Mountains in the watersheds of the Niobrara, Platte, Kansas, and upper Arkansas Rivers. Lying within what was once termed the Great American Desert, it includes the present states of Nebraska and Kansas, plus eastern Colorado. Not all sections of this area are equally well-known archeologically, but the broad outlines of human prehistory are fairly well established (Strong 1935; Wedel 1940, 1947; Champe 1946). The geographic limits I have set are largely for convenience in handling our immediate problems; they are more or less arbitrary and do not necessarily conform to any natural or cultural boundaries.

The salient features of physical environment in the Central Plains, as they relate to archeology, have been set forth in detail elsewhere (Wedel 1941, 1947a). Generally speaking, it is a land of low relief, few trees, and highly variable rainfall. The climate is of continental type, with cold dry winters, hot summers, strong wind movement, abundant sunshine, and marked daily and seasonal fluctuations of temperature and precipitation. Except in the extreme northwest corner, the area has a frost-free growing season of 100 days, or more, in four years out of five.

Before the day of intensive agriculture, the eastern portion consisted of

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subhumid tall-grass prairies interspersed with numerous sparsely to heavily wooded watercourses, and with precipitation averaging from 25 to nearly 40 inches annually. Westward, the prairies give way to mixed grasses and then to short-grass steppe, where rainfall diminishes to 15 inches or less, evaporation increases, and surface water over large areas is scanty or absent for much of the year. Very roughly, the 100th meridian may be taken as the dividing line between prairies and plains (Fig. 1); but like all natural boundaries in the region, this one is transitional rather than abrupt. To the east, less than 10 years in 20 are characterized by sub-average precipitation and the subsoil tends to remain permanently moist. To the west, more than 10 years in 20 have sub-average precipitation and the subsoil is permanently dry except where, as in the Nebraska Sandhills, loose-textured sandy soils permit deeper penetration of the limited rainfall and thus are able to support a mixed or tall-grass flora (Trewartha 1941; Thornthwaite 1941).

Origin of the North American grasslands, including prairies and plains, is beyond the scope of this discussion. It may be noted, however, that some ecologists are of the opinion that the prairie is an induced grassland growing in a climate suitable for tree growth, which was prevented in pre-white days by such agencies as fire and buffalo (Trewartha 1941: 171; Shantz and Zon 1924: 16). Sauer (1944: 543–554) has argued at some length that the grasslands are "late features of plant geography," perhaps with fire the instrument and man the agent by which they were created (see also Stewart 1951). On paleobotanical grounds, Elias (Sixth Plains Archeological Conference 1950: 134) has re-affirmed his belief that they long antedate man and have a geologic and climatic basis. At least one pedologist, Thorp (1948: 61), holds that in the drier soils west of the true prairie " . . . it is doubtful whether any extensive forest could establish itself . . . without the aid of man. . . . "

That the prairie-forest boundary, like that between the prairie and steppe, has fluctuated throughout the centuries is very probable. So far as the archeological record and associated data are concerned, however, I know of no proof that environmental conditions over most of the Central Plains during the past four or five thousand years, at least, have differed radically from those of the past century or two—a basically grassland region with belts of timber confined largely to stream banks, valley margins, and broken terrain.

With respect to native subsistence economies, perhaps the most important environmental consideration was climate. For peoples depending partially or largely on domestic crops, the rainfall factor was of prime significance. The line of 20-inch average annual rainfall, about the minimum required for dependable agriculture except by specialized methods, runs erratically north and south between the 100th and 101st meridians. Unfortunately for the food-producer, this 20-inch line may shift as much as 200 miles to the east or west in accord with precipitation deficiencies or excesses (Fig. 1). Thus, in an ex-

ceptionally wet year, as in 1905, much of the Kansas-Nebraska region may have a climate approaching that normally characteristic of Iowa and western Illinois. In a very dry year, the same localities may be little better than desert. These fluctuations are of variable duration and intensity, do not occur in regular succession, and cannot be accurately forecast. They are by no means peculiar to the region; but because the average precipitation is so close to minimum crop requirements, any appreciable departure from that average may quickly determine the success or failure of agricultural efforts.

Other complications arise from the fact that droughty conditions in summer are frequently accompanied by prolonged periods of excessively high temperatures. These may reach shade readings of 100° to 110°, or even more, and are often associated with hot, drying winds. Such temperatures are extremely damaging to corn, especially at flowering time; and in combination with the hot winds they rapidly exhaust the scanty moisture contained in the soil. When the hot winds occur at critical stages in crop development, they may do untold harm in a few hours, besides causing great discomfort to animal life; if they continue for several days, widespread crop failures ensue.

Climatic fluctuations are not recent phenomena in the Central Plains; they are a function of the geographic location of the area in the border zone between humid and arid regions. History bears testimony to the fact that these uncertainties troubled the Indian, as they have the white man, in the Central Plains.

HISTORIC SUBSISTENCE ECONOMIES

In historic times, native peoples exploited this region in various ways. During the 1800's, the short-grass plains were the range of the great bison herds. Closely dependent on these was a group of roving tribes which may be designated the migratory bison hunters. These included the Comanche, Kiowa, Cheyenne, Arapaho, Dakota, and others beyond our immediate area. From the spring of each year until fall these Indians followed the herds, subsisting principally on the flesh of the bison and drying great quantities of meat for winter consumption. Hunting practices included cooperative methods —grass-firing, the surround, and presumably also impounding and driving over cliffs. Winters were spent in sheltered spots where water, wood, and forage for the horses were available. The portable skin tipi and the travois were universally used; skin-working was highly developed; agriculture and pottery-making were virtually non-existent; and implements and utensils were limited to essentials that could be easily transported by dog and horse traction from camp to camp. Wild fruits, nuts, berries, and roots, both fresh and dried, supplemented the meat diet; corn was obtained by barter or theft from other tribes who raised it. No fixed tribal boundaries were recognized, and the distances ranged over by the various tribes in their hunts and raids often to-

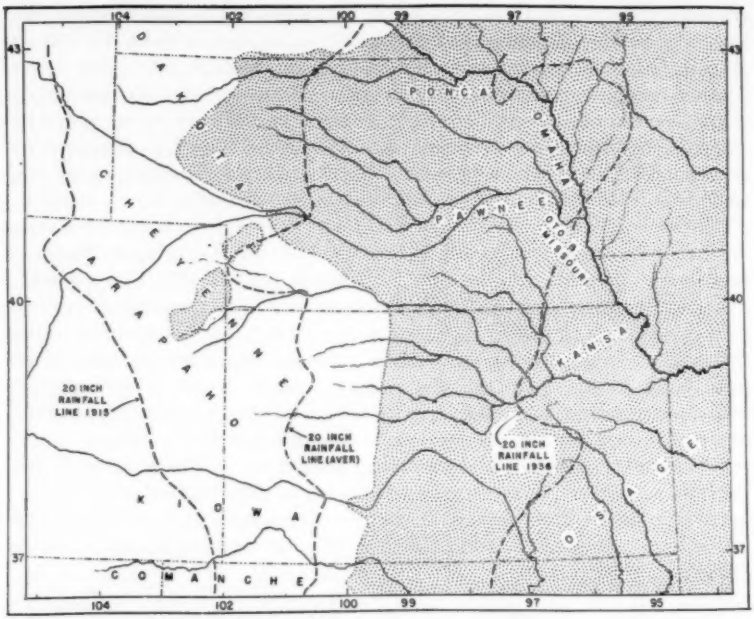

FIG. 1. The Central Plains. Broken lines, 20-inch annual rainfall in average, wet (1915), and dry (1936) years. Stippling, tall grass prairies (bluestem, bunchgrass, etc.); unstippled, short grass plains (grama, buffalo grass). Tribal locations as of approximately 1825.

talled many hundreds of miles. These peoples, for the most part comparatively late arrivals in the region, were the "typical" plains Indians of the historic period.

East of the 99th meridian, since the coming of the whites, have dwelt chiefly Siouan- and Caddoan-speaking tribes—the Kansa and Osage in eastern Kansas and adjacent Missouri, the Oto, Missouri, Omaha, and Ponca in eastern Nebraska, and the Pawnee on the Loup and Platte Rivers of east central Nebraska. At the beginning of the white contact period in the mid-16th century, there was a group of Wichita settlements in the Arkansas drainage of central Kansas, but these were apparently given up by or during the early 18th century. All these tribes dwelt in more or less fixed villages of earth-, grass-, or bark-covered lodges situated in or near large stream valleys where water, wood, arable land, and pasturage for horses were readily available. Maize, beans, squash, sunflowers, and perhaps other crops were cultivated; wild fruits, nuts, berries, and tubers were gathered in season; and once or twice

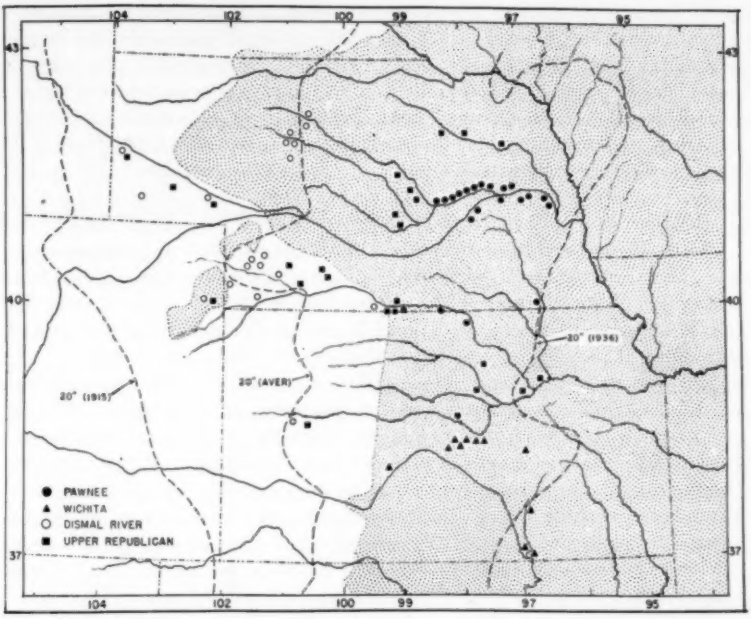

FIG. 2. Distribution of principal known semihorticultural complexes in the Central Plains west of the Missouri River, as indicated by archeology, showing relation to fluctuating rainfall (broken lines) and to grassland divisions. Symbols indicate localities of occurrence rather than specific site locations.

each year the entire able-bodied manpower of the village departed on a lengthy communal bison hunt. During these excursions, the people lived in skin tipis, hauled their equipment on horseback or travois, and lived in general like the migratory bison hunters. Before 1800 all these tribes made pottery, and had fairly well-developed industries in stone, bone, horn, and other materials.

On the historic level—say the year 1800—there is an interesting correlation between the native subsistence economies as just outlined and the environmental subdivisions of the Central Plains (Fig. 1). So far as present knowledge goes, the permanent settlements of the primarily horticultural, or Village, Indians of the Nebraska-Kansas region all lay east of the 99th meridian. In other words, in terms of the natural environment as the white man knows it, they were within the subhumid tall-grass prairies and well to the east of the line of 20-inch average annual rainfall. To the west, in the High Plains or short-grass country where rainfall averages 16 inches or less, were the non-agricul-

tural Comanche, Kiowa, Cheyenne, Arapaho, and Dakota. With possible exception of the Cheyenne when they still centered their activities in and around the Black Hills, none of these tribes made a serious effort at cultivation in its historic habitat. In short, the peoples among whom food-producing was of considerable or primary importance had their regular residence in the agriculturally dependable region; those who were primarily food-collectors occupied the agriculturally unsafe lands.

FOOD-COLLECTORS, FOOD-PRODUCERS, AND PHYSICAL ENVIRONMENT

In the light of archeology, it is clear that these two basic subsistence economies, or variants thereof, existed in the Central Plains for a long time before the coming of the white man. The relative emphasis placed on food-collecting (*i.e.*, hunting and gathering) as opposed to food-producing has varied, however, with different peoples and at different times. In the perennially dry far western plains, hunting and gathering must have been at all times the usual mode of subsistence. Farther east, food-producers tried their hand at making a living; and we know that in pre-white days peoples relying in part on corn-growing lived for a time far to the west in what is today a semi-arid short-grass country. Despite the still imperfect nature of our present knowledge of culture developments here, there are data at hand that suggest correlations between certain cultural horizons and, inferentially, an improved climatic setting. Let us review briefly the history of these two basic modes of life as they are revealed by archeology and other lines of evidence.

First, with respect to the western plains. The old thesis that these constituted an uninhabited tract before introduction of the horse is, of course, thoroughly out-dated. In my opinion, the somewhat more recently stated view that its pre-horse inhabitants were merely occasional would-be agriculturists straying out of the eastern woodlands or else hunting parties venturing forth from a more secure habitat in the western foothills, will soon be equally dated. Man, it is fairly clear, has existed in these grasslands for some thousands of years. His earliest traces—the Paleo-Indian remains—seem to be most plentiful near the mountain front; but surface and subsurface finds in eastern Colorado, western Kansas, Nebraska, and elsewhere certainly show that hunting and gathering groups were camping far out on the High Plains proper five to ten thousand years ago. For the most part, these were undoubtedly small roving bands whose sustenance came primarily from the large game mammals. To what extent they hunted by organized drives, utilizing fire, falls, or pounds as did some of the pre-white bison hunters farther north, we cannot be sure; but there is evidence of mass slaughter. In all likelihood, wild vegetal foods were also used extensively. The widespread occurrence of such early remains and sites suggests that these foot-hunters were crossing and re-crossing the plains on a considerable scale.

Concerning the inhabitants of the western plains between the time of the Paleo-Indian and that of the historic hunters, we as yet know relatively little. Innumerable sites, generally small and obscure and seldom very rewarding to the archeologist, have been revealed by the sustained surveys of recent years. They occur throughout the short-grass plains wherever suitable camping localities were at hand—in stream terraces, rock shelters, and elsewhere. The record is growing steadily.

Before arrival of the horse, subsistence on the herds plus the gathering of wild vegetal products must have meant a rather uncertain and impoverished existence by comparison with the sometimes colorful life of post-horse days. Nevertheless, the narratives of Coronado (Hammond and Rey 1940) and Oñate (Bolton 1916) tell us that roaming peoples "who lived like Arabs" and "whose sustenance comes entirely from the cows because they neither sow nor reap corn" were living in the southern plains in considerable numbers and apparently in fairly sizable communities in the 16th century. Their custom of wintering near the settlements of the sedentary peoples, as well as their documented trade relationships with the pueblos to the west and other maize-growing Indians to the east, whereby meat and hides were exchanged for corn and other items, are reminiscent of the later historic interactions between hunter and agriculturist. Dog traction, skin tents, and a host of other traits mentioned suggest a mode of life basically like that of the later plains hunting peoples.

All of this seems like good evidence that, long before the coming of Europeans and the horse, man had developed basic subsistence techniques that enabled him to survive and function in the creek valleys and around the waterholes of the short-grass plains. So far as the intermittently watered upland portions of the High Plains were concerned, the indicated occupations could hardly have been on a year round basis, if we assume climatic conditions substantially like those of the present. Cold weather and dry season movements to valley bottoms and to broken, hilly, or mountainous localities offering shelter, water, and wood were probably customary, as they were for the historic bison-hunters of the region.

Our main concern here, however, is with the native peoples who at various times in the past have attempted food-producing between the Missouri and the Rockies. Just where, when, and how this important step was initiated, we cannot yet say certainly. That it was somehow linked with one or another of the earliest ceramic cultures, that is, with Woodland peoples, seems fairly probable. Whether it began with a maize-bean-squash assemblage or with something more rudimentary based primarily on cucurbits (Strong 1935: 268), the evidence likewise does not yet tell us conclusively.

With respect to their basic food economy, the Plains Woodland peoples will perhaps have to be classed for the present as primarily food-collectors,

only in part or in their later stages with incipient horticulture. Their sites are generally small and inconspicuous, often deeply buried in stream terraces; structural remains are scanty and inconclusive; pottery and other artifacts of durable materials are not plentiful; and tools clearly designed for horticultural purposes, as well as underground pits for crop storage, are rare, absent, or unrecognized. Animal bone refuse suggests a greater reliance on deer, small mammals, and birds than on bison (Strong 1935: 268; Kivett 1952); but this, of course, may reflect hunting practices or butchering techniques rather than actual food preferences.

On the whole, most Woodland manifestations in the Central Plains look primarily like a creek valley hunting and gathering economy, characteristically with small population aggregates—perhaps limited family groups. Even where these remains are found far out in the short-grass plains, the picture is much the same—a mode of life, one thinks, that was probably carried west out of a forest margin habitat into the tree-fringed creek valleys of the plains. Probably the step from this economy to a purely bison-hunting pattern would not have been a particularly difficult one, especially under the proper climatic or other stimulus. One wonders whether, in part, the peopling of the western steppe by confirmed plains hunters may not have taken place in some such fashion.

On a later time level, though still in the prehistoric period, the Upper Republican culture and contemporary manifestations suggest a markedly different picture. Here again sites are usually situated along the lesser watercourses. Communities were small, loosely arranged, and unfortified, with usually a half dozen to perhaps a score of earth-covered dwellings. I doubt that many of these settlements, often scattered over several acres of ground, consisted of more than one or two hundred persons; most were doubtless considerably smaller. For such small aggregates, the usually limited creek bottoms furnished adequate nearby garden space; and game animals, gregarious and other, were readily available to foot-hunters in the valley bottoms and upland margins.

There is no question that horticulture was important among these small-town creek valley peoples. The maize-bean-squash-sunflower complex recurs, and the bone hoe is a characteristic, if not ubiquitous, trait. Storage pits are usually present; but they are small and seldom occur in any numbers. This may imply that the small scale horticulture was little more than a year to year proposition, with limited crop surpluses and scanty reserves. Unfortunately, we still know very little about the kinds of maize in use among these peoples.

To me, the relatively permanent habitations plus the indubitable evidences of horticulture on the Upper Republican level are indicative of a much more firmly established and more sedentary occupation than that inferred for the earlier Woodland occupation. Moreover, these small partly horticultural,

partly hunting-gathering communities apparently existed at one time as far west as the plains of eastern Colorado, far beyond the western margins of successful native horticulture in historic times, and about as far west as maize can be grown except through highly specialized methods (Fig. 2). From the abundance of these remains along almost every arable stream valley with dependable water west to, or beyond, the Colorado line, I doubt that they were merely "strays" from better watered lands to the east. On the contrary, I suspect that they were people with horticultural traditions of long standing who spread westward through a climatically favorable environment that represented something quite different from a one- or two-year westward swing of the rainfall zones. As yet, we know virtually nothing regarding the subsistence economy followed by these people in the extreme westerly portions of their known range; but the sites are there, and the essential details can undoubtedly be cleared up through properly done field work.

Many of the Upper Republican village sites, especially west of the 99th meridian, appear to be associated with old soil horizons that almost certainly represent former humus lines and land surfaces subsequently mantled with aeolian deposits; and it is also possible that the overlying materials reflect a period of diminished rainfall and renewed soil movement that may have been a factor in abandonment of the villages (Wedel 1941, 1947a). Whatever the cause or causes, it is clear that at some time before the white man arrived, the westerly earthlodge villages had been given up; whether their inhabitants retreated eastward and clung to their horticultural practices or gave up the latter and turned to nomadic bison-hunting, I cannot say. Perhaps both alternatives were taken. At any rate, the earliest whites in the area apparently found no horticulture west of, approximately, the 99th meridian.

In earliest white contact days, during the 16th century, the horticultural communities of the Wichita and Pawnee were a few large villages instead of many small creek valley communities. The protohistoric Pawnee sites of the Lower Loup focus are characteristically in or near the large stream valleys; those of the Wichita of the Great Bend aspect are sometimes on small creeks and at other times in the large valleys. Generally, they seem to be of greater size, suggesting larger population aggregates, and they were occasionally fortified in part. The Oneota sites on the eastern margin of the area look much the same. There seems to have been a very intensive and productive horticulture, however, for the storage pits of this period, especially those of the Pawnee and Wichita, were often enormous—up to eight and ten feet deep and of equal diameter—as well as very numerous. The incomplete archeological record offers confirmation of Oñate's account of a flourishing and productive corn agriculture on the middle Arkansas in 1601; and charred ears of corn from this period in central Kansas indicate the presence of well-developed maize varieties.

All this suggests a determined and apparently temporarily successful effort at increased food production, perhaps aided by recently developed or newly introduced heavier-yielding crop varieties, more efficient tillage methods that may have fallen into disuse later with taking over of the horse, a temporary bettering of climatic conditions, or perhaps a combination of these and other still unrecognized factors.

Regarding the 16th century occupation of the Central Plains west of the 99th meridian, we have no direct documentary data. Since Plains Apache peoples were then residing in the southern High Plains it can be assumed perhaps that they were also living in the plains farther north. Archeological sites certainly assignable to this period are lacking. Somewhat later, around the end of the 17th century and in the early 18th century, the region that is now northwestern Kansas, western Nebraska, and eastern Colorado was inhabited by semihorticultural peoples of the Dismal River culture (Hill and Metcalf 1941; Champe 1949). I believe these were also Plains Apache; but whether the Dismal River sites represent the remains of an erstwhile wandering hunter folk who had taken up a desultory maize agriculture under stimulus from 17th century puebloan fugitives from the upper Rio Grande, I am not prepared to say at this moment. Most of the archeological evidence bearing on this culture is still unpublished, but probably their creek-bottom horticulture was of secondary importance and definitely subordinate to bison-hunting. Whether the roasting pits found on these sites reflect some specialized root-food complex or were used in connection with meat preparation is not clear. There is no precise information about the dwelling units and population of these communities, though I suspect they were not as tightly crowded as were the Pawnee villages farther east, nor do they appear to have been fortified.

By the 1800's, as among the Pawnee, further changes had taken place. The prehistoric small-town creek valley culture of Upper Republican days had been replaced by a big-town river valley culture. Characteristically, towns were few in number, situated on or very near the immediate banks of such large streams as the Platte, Loup, and Republican, included from 50 to 200 or more earthlodges, and contained from 1500 to 3000 inhabitants. They were well fortified and compactly built; while I have no precise figures, it seems likely that houses were sometimes clustered as tightly as five, eight, or more to the acre—perhaps ten times the density of the earlier Upper Republican communities.

Settlements of this size obviously would require many hundred acres of croplands. Tightly sodded ground resisted cultivation with the primitive tools available, and such was the pressure on tillable bottomlands that some of the garden plots were located five to eight miles distant. The food thus produced was evidently insufficient to meet minimum food requirements, and so was supplemented by the products of the chase. The subsistence economy of these

late peoples combining horticulture with hunting and gathering, was, of course, very considerably influenced by the horse. This facilitated hunting over a wide area as the nearby game was exhausted, and at the same time brought the people into armed conflict with other groups primarily depending on the bison for their sustenance.

With regard to native horticultural peoples in the Central Plains, the archeological record, then, would seem to indicate a progressively more intensive and thorough utilization of environmental opportunities throughout the last few hundred years of prehistoric time. At least once in that record, and perhaps several times, these peoples carried their food-producing economy westward to the extreme limit of maize culture in the region. In historic times, it is quite probable that the westward spread of agricultural practices was limited as much by the presence of hostile nomads in the short-grass country as by climatic uncertainty. The growth of large towns in the eastern plains may have been in large part due to the need for defense; and the obvious trend toward fortification of 18th century and later settlements seems confirmatory. At the same time, it may be questioned whether native horticultural techniques would have permitted survival of large semisedentary communities, even if fortified, in the semiarid western plains. While the gathering into large communities of several thousand people in historic times may well have been enforced by cultural factors, it was made possible in the eastern plains by a more favorable horticultural environment, the presence of large amounts of tillable land in the great stream valleys, and consequently better assured food resources.

It is perhaps worth mentioning here that the concentration of native population into a few large communities within short distance of each other, besides affording greater security against hostiles and complicating the food problem, must have been an important stimulus to development of the tribal and intervillage ceremonies and institutions which characterized Pawnee life in its early post-white heyday. On the debit side it also exposed these populations to heavy losses from contagious diseases introduced by the white man. I doubt that the sparse populations living in small widely dispersed bands in the western plains, or even the scattered villages of the Upper Republican peoples, would have been so seriously affected by epidemics as were the tightly crowded communities of the Village Indians in the historic period.

Since native horticultural practices of the Central Plains Indians were basically a subsistence economy, as contrasted to the fundamentally commercial orientation of the white man's agriculture, the validity of the 20-inch rainfall line in my discussion of native horticultural practices and potentialities may be open to question. I have used that line as a point of departure for two reasons. For one thing, it seems to have general acceptance in present-day discussions of land-use problems; for another, its fluctuations over the years during which recorded observations have been in progress strikingly

illustrate the highly variable nature of precipitation. That it very likely has relevance to the present problem—and indeed the validity of the climatic factor in the history of native horticultural economies in the Central Plains—can perhaps best be brought out by a few observations from the historic period. This point has been discussed elsewhere, but additional data are now in hand.

The drought of 1860, considered one of the most severe since white settlement began west of the Missouri, gravely affected nearly all the horticultural Indians from east central Nebraska southward through eastern Kansas into Oklahoma (Commissioner of Indian Affairs 1860). Throughout this region, partial or total crop failures occurred almost everywhere; and the reports of the agents make clear the fact that it was subnormal rainfall, high temperatures, and hot winds that did the mischief. It was not alone the planted crops that suffered. In many localities, even the hay was burned up; and in the normally well-watered southeast section of Kansas, where annual precipitation averages nearly 40 inches, streams ceased running and wells and springs dried up. Precipitation records for the period and area involved are extremely scanty, but the few available are suggestive. The following table lists such figures as I have been able to trace down; it includes the 1860 rainfall, the normal rainfall, and that recorded at the same stations during the recent drought of 1936:

Station	*1860*	*1936*	*Average*
Fort Laramie, Wyo	3.48″	14.98″	13.04″
Fort Kearney, Neb	16.85″	13.53″	24.71″
Fort Leavenworth, Kans	19.19″	24.01″	34.81″
Manhattan, Kans	15.13″	24.54″	31.49″

The rainfall figures for the easternmost stations here given—those nearest the Indians most seriously affected by drought—suggest an 1860 drought rivalling or exceeding in intensity that of 1936. If we can trust the observations of the Indian agents, there would seem little reason to doubt that if the 1860 conditions had continued for a few years, the Indians would have had to move out altogether or perish. And this would have happened in an area where rainfall averages dropped below 20 inches annually. It thus appears that even the small scale valley bottom horticulture of the Indians was vulnerable to rainfall deficiencies, high temperatures, and hot winds. Similar effects could be expected in the still less reliable western plains localities where the Upper Republican peoples three or four hundred years previously must have encountered similar unfavorable climatic conditions from time to time.

We will probably never know for sure how far west corn-growing could have been carried on by the 19th century Indians, had they followed the mode of life inferred for the Upper Republican horticulturists. Plains Apache groups in

early 18th century seem to have attempted small scale farming well to the west of the 99th meridian, apparently with some success; whether this could have continued if the Comanche had not dispossessed the Apache, we can only speculate.

The buried soil zones seen in many stream terraces west of the 99th meridian look like indicators of periods of increased rainfall and vegetative growth as compared to the intervening and overlying culturally sterile aeolian deposits. Moreover, the aeolian deposits strongly suggest that locally, at least, extensive soil blowing took place long before the plow broke the grasslands but since human occupancy of the region began (cf. Clements 1938: 202). How the climate indicated by these zones—specifically, those bearing traces of human activity—compared with the present one I have no way of knowing. What is clear is that some of these periods were of such character that corn-growing peoples lived here for a time, and that conditions of lesser vegetative growth and increased soil movement followed during which no corn-growing was carried on.

The climatic fluctuations thus suggested may have been of greater intensity and longer duration in prehistoric times than were the relatively short-lived phenomena that have visited the region since the white man entered it. The spread westward of early Woodland hunters and of later maize-growing peoples may have been encouraged by a sustained westward shift of agriculturally favorable climatic conditions, that is, of rainfall zones, only to be discouraged by recurring drought periods. From the standpoint of food producing and in the long range view, native man's hold on the western plains at and beyond the edge of the tall-grass prairies, must always have been an uncertain one. In the face of drought, hot winds, dust storms, and perhaps insect infestations, these marginal horticulturists would doubtless suffer as did the later Indians and their white successors. In pre-horticultural days climatic variations may well have been yet more pronounced. The deep loess deposits (Schultz, et al. 1948) overlying some of the pre-ceramic sites on Lime Creek, for example, suggest very severe and prolonged periods of drought and high wind activity which would probably have made the locality undesirable or impossible as the abode of man.

SUGGESTIONS

There are several avenues of approach to a clearer understanding of the fascinating but still vexing problem of man versus nature in the Central Plains. For one thing, we need a careful analysis of community patterns, economic practices, size of population groups, and relative emphasis on food-collecting as against food-producing on the part of the historic Village Indians. Of equal importance is a thorough-going ethnographical and ethnohistoric synthesis and interpretation of western Plains Indian culture as it must have been in pre-white and pre-horse days. Ethnography is in a good position to

contribute much useful information along these and other lines (cf. Eggan 1952: 39–41).

Similar analytical studies will have to be made of the earlier populations, communities, and their subsistence bases, as the results of intensive excavation programs become available. This will involve closer attention to the floral, faunal, and other geographical associations involved, which means, in turn, more help from specialists in other disciplines.

With regard to the interrelationships between archeology and pre-historic climates, we will need the full cooperation of competent workers in geology, pedology, and climatology. The climates of the period preceding instrumental observations will have to be determined largely from the land itself, as expressed in terrace and cave fills, soil profiles, and otherwise. These prehistoric soils and land surfaces ought to be datable from the archeological materials in or on them, and it should be possible for properly trained observers to correlate them from locality to locality. From this we may get a clearer understanding of the magnitude and nature of the indicated climatic phenomena and their relevance to man's activities.

The establishment of a more precise and broader chronology here is also necessary. Dendrochronology may or may not help to determine the time of the prehistoric occupations; it should give us some indication as to how long they may have lasted. The application of Carbon 14 techniques is indispensable if these climatic interpretations and correlations are to be extended very far back into the 10,000 years or more of human existence in the Plains. Among other things, it will be interesting to see whether the archeological sites and complexes in the western plains, as acceptable dates are assigned them, show clustering or grouping at certain time levels. The gap that once seemed to exist between geologically dated Early Man and the archeologically dated later culture horizons is steadily narrowing or breaking up. It does not yet follow, however, that there may not have been shorter interludes of non-occupancy, perhaps climatically induced, in considerable sections of such regions as the western Great Plains.

Our insights into the interactions of man and his natural environment in the Central Plains are still extraordinarily incomplete. We have some very good clues but lack the details. It should be fairly obvious that we are not likely to get far in our interpretations until we combine our labours with those of specialists in other fields and make a sustained, purposeful effort. Moreover, I am of the opinion that our interpretations of culture history will have to be premised on a dynamic and ever-changing picture—environmentally as well as culturally—instead of on armchair views and preconceptions as to what native man could or could not have done with what he had.

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THIS ISSUE, AND OTHERS

(*Continued from page 480*)

veniently available to your editors. (We exploit colleagues wherever found but those at home institutions are particularly vulnerable).

The Symposium on the page opposite is the second of a series—although a third is not yet in sight. In the August issue scholars from various fields discussed the paucity in our language of words for process alleged by Earl Count, who suggested this proved something about our historic culture. Eliseo Vivas (Northwestern) and F. S. C. Northrup (Yale) are philosophers with long interest in anthropology; James Sledd (Chicago) is in English linguistics; and G. G. Simpson of the American Museum of Natural History is the theorist of biological evolution. They joined David Bidney, Dorothy Lee, and Count in the discussion in that first symposium.

The variety in the second symposium—not quite a discussion over beer—is no less striking. Besides Braidwood (Chicago), Coon (Pennsylvania), Linton (Yale) and Steward (Illinois), there are the botanists Jonathan Sauer (Wisconsin), Paul Mangelsdorf (Harvard) known to anthropologists for his work on maize, and Hugh Cutler (Missouri Botanical Gardens and Washington University). Leo Oppenheim is the Near Eastern philologist (Oriental Institute, Chicago) who has given the most recent attention to the matter of brewing. Hans Helbaek is curator of prehistoric agriculture at the Danish National Museum.

We seem to be heavy this issue on "ecology" what with Wedel's article on the Plains following so closely the B-B piece. Might the word or concept be competing for popularity with the anthropological stand-by, culture? The Bagby article (p. 535 et seq.) ought to redress the balance. For sending the author to us we are grateful to A. L. Kroeber, who first saw Bagby's article. After accepting it we asked the author (then at the American Academy in Rome) who he might be, and he told us: Harvard (Philosophy B.A.), the

(*Continued on page 526*)

SYMPOSIUM: DID MAN ONCE LIVE BY BEER ALONE?

This is the second in a series of symposia the object of which is to expedite scholarly intercommunication. Robert Braidwood's query was referred to several colleagues for comments which were sent to Braidwood for rejoinder; then the series was edited as a whole. Similar queries in any field of anthropology, or at the margins, are invited, and the author may suggest names of persons whose comments he would value.

QUERY *by* ROBERT J. BRAIDWOOD

It is generally assumed that the appearance of domesticated cereals in the Near East was intimately linked with the use of these grains for the preparation of flour for bread making. Recently Professor J. D. Sauer of the Department of Botany of the University of Wisconsin has queried this generalization in relation to our preliminary notices on Jarmo *et al.* (cf. Braidwood: *Scientific American*, Oct. 1952, pp. 62–66; *The Near East and the Foundations for Civilization*, University of Oregon Press, 1952). Sauer wonders whether the earliest utilization of the domesticated cereals may have been for beer rather than bread. The question would thus appear to be: Could the discovery that a mash of fermented grain yielded a palatable and nutritious beverage have acted as a greater stimulant toward the experimental selection and breeding of the cereals than the discovery of flour and bread making? One would assume that the utilization of *wild* cereals (along with edible roots and berries) as a source of collected food would have been in existence for millennia before their domestication (in a meaningful sense) took place. Was the subsequent impetus to this domestication bread or beer?

The contemporary archeological evidence from the area which bears on the point would be, in order of appearance: Natufian of Palestine—sickles (reaping, but no direct implication of planting), mortars and pestles (mortars small, with conical sections, probably for ochre or bone grinding); Karim Shahir of Iraq adds—celts (used as hoes, or as axes, or both?); Jarmo of Iraq (and in fair part Jericho XVII—X of Palestine) adds—grain (two wheats and a barley, in process of being bred), querns and rubbing stones, built clay ovens with smoothed fire-hardened floors, receptacles in the form of baked-in place clay floor basins, stone vessels, and bitumen-coated basketry (??); uppermost Jarmo of Iraq and Jericho IX of Palestine add—pottery. From this point onwards the familiar early village sequence proceeds.

It will be seen that this direct evidence, as now available, is of no particular use in solving the question either way. The cereals themselves, and the tools for planting and reaping, would be involved for either the bread or the beer: so also, probably, would the milling stones. The receptacles for liquids obviously need not imply beer alone. When the grain itself is recovered (and not

the hollow casts of grain in clay, which Helbaek has so successfully used for identifications) the kernels are invariably charred. A question arises as to whether these charred kernels may have been overfired accidentally in the parching process by which the malt was prepared, although I have seen grain parched for other purposes.

I am myself in no way unattracted to the idea, as Sauer put it, that "thirst rather than hunger may have been the stimulus behind the origin of small grain agriculture." Do any of our colleagues recall tools or processes in either beer or bread making which might be expected to persist in the archeological record and which would lead to further understanding of the point?

Comments

by Jonathan D. Sauer

The pioneer cultivators of wheat or barley must have been faced with a pitifully small return of grain for their labor. Yields of the primitive grain crops must have been poor, even after the development of cultivated forms which retained the ripe grain in the head, instead of shattering the inflorescence and scattering the grains on the ground when ripe, as do the wild wheats and barleys. Planting and harvesting small grains without the plow or other efficient tools would seem to me a game scarcely worth the candle except for a more rewarding stake than mere food.

One of the most suggestive pieces of evidence is the constant confederacy of barley and wheat in the early finds in general, encountered again at Jarmo. In primitive agriculture a dual grain crop is remarkable enough to require some explanation and it seems to me easier to account for in terms of a beer recipe than in terms of carbohydrate foodstuff.

Of course it is possible that in the beginning only one of these grains was the deliberately planted crop, while the other was tolerated as a volunteer which had entered the fields as a weed. It is clear that during the Neolithic and later times, as the cultivation of barley and wheat spread, various local grasses moved into the fields as weeds, migrated with the crops, and were gradually modified by accidental hybridization and selection in the normal processes of cultivation and harvesting. The story is well known of how such unplanned events among the weedy invaders of barley and emmer wheat fields gave rise to domesticated oats and rye as well as to the more recent wheats and barleys.

It takes little imagination to extrapolate this process back to earlier times and suggest the possibility that the entire complex of western small grain agriculture may be the outcome of just one case of deliberate domestication of a wild grass. If so, barley may have been the original crop and cultivated wheat may have evolved from weed grasses in the barley fields. Recent botanical evidence suggests that the original home of the first domesticated barley was

not in the Near East but the Far East, possibly Tibet. Thus it is conceivable that the lowest agricultural levels in the Near East represent a stage in the westward movement of an agriculture which had started well to the east, perhaps with the domestication of barley, and had picked up wheat as it spread into the Near East. This possibility does not detract from the thesis of brewing as the basis of early agriculture, since it is barley that is critical in making beer and wheat is quite dispensable.

Incidentally, a corollary of this argument that may not occur immediately to everybody is that the oldest domesticated plant may be yeast.

by HANS HELBAEK

The suggestion put by Professor Sauer and commented upon by Professor Braidwood raises a most fascinating problem—if a most unapproachable one. In spite of the deplorable fact that the Jarmo material does not in any way sustain a discussion along these lines I shall be pleased to comply with your wish for a comment.

One would spontaneously conceive brewing as later than bread making—in its widest sense—in the technological sequence, in the same way as one would be apt to suggest that the first cattle were domesticated for food—meat and milk—rather than for making intoxicating beverages from the milk. Implements for brewing would hardly be specialized to such a degree in these early days that we could determine their exact purpose, to the exclusion of other alternatives. Of course we may point out that a large vat would be useful in the malting process, but we cannot exclude its possible employment otherwise, and a stick might be perfect for stirring the malt when drying—but, well, now I am touching the sore point of archeological interpretation. Even the presence of germinated grain should not inevitably lead to the conclusion that it is malt, as germination may well happen unintentionally in consequence of unfavorable storage conditions. The interpretation of germinated grain found in prehistoric sites may depend upon a complex of criteria.

As for the carbonization as such being an indication of activities connected with brewing, I would like to point out that every deposit of grain but one known to me contradicts this view. If you will allow the space I shall try to explain this point.

Theoretically brewing consists of the conversion of starch into fermentable saccharine compounds which eventually, by the action of yeast organisms are divided into alcohol and carbon dioxide.

In practice brewing from grain is performed in two stages, the malting and the fermentation. The former process deals with the living grain, in the latter the grain as such is killed. Only the first or malting process is relevant to the question in hand.

The conversion of the main substance of grain, the starch, is brought about by stimulation of the natural germination of the grain. By generous administration of moisture and fresh air, and moderate heat, the germination is started. During this process the grain itself develops an enzyme, diastase, which converts the starch into fermentable sugars. After a few days the germination is discontinued and the germ is killed by heat—but nothing amounting to carbonization, which would be destructive to the purpose of brewing. The subsequent measures would hardly be identifiable in carbonized material, and we shall confine ourselves to considering the malt.

As carbonization cannot be employed in malting, carbonized malt must be the result of an accident, and if, as in Jarmo, we find only ungerminated grain it does not bear on the point at all. Carbonization may happen in a multitude of ways, for instance by the grain having been spilled into the ash at the fireplace during cooking activities. In prehistoric times—as still today—house fires afforded beautiful opportunities for large quantities of grain to be carbonized. In this connection it should be pointed out that only the dry desert climate, as for instance in Egypt, would allow farinaceous plant parts to be preserved if not carbonized. In Jarmo all uncarbonized grain would be destroyed in a short time by putrefactive microbes, and this would be the case in any area in which the climate allowed agriculture on basis of the rainfall. For this reason the "spontaneous combustion" of grain, now and again postulated in the archeological literature, is pure nonsense. Fire must always have been involved.

However, the priority of bread making or brewing as the stimulant towards plant breeding is a problem much too complicated to be solved simply by the interpretation of archaeological material and subsequent speculations. Folklore and ethnology must be drawn into the discussion to pay the matter due respect.

As two of the foremost European authorities on the subject of preparation and utilization of cultivated plants in the past and in various parts of the world, A. Maurizio, the Polish agricultural botanist, and Gudmund Hatt, the Danish ethnologist and archeologist, have contributed to the discussion of the question in an excellent way, I can do no better than refer to their conclusions.

Maurizio (in *Die Geschichte unserer Pflanzennahrung von den Urzeiten bis Zur Gegenwart*) expresses the view that the pouring of water upon a vegetable foodstuff, making some kind of suspension, extract or solution (*Aufguss*), is an elementary and exceedingly old way of preparing food, and almost all kinds of dishes originate from this elementary process. One line of development leads via gruel and porridge to bread; another line, employing fermentation, leads to beer, wine, and stronger alcoholic beverages.

If brewing was the primary utilization of cereals from the beginning it should be expected that diffusion of agriculture first and foremost was mani-

fested by spreading knowledge of brewing. But this, as shown by numerous examples, was not the case. Let me quote an example from Gudmund Hatt: "In the southern part of Central America and in South America maize is used not only for bread, but also for making beer. A kind of malt is made by letting the maize sprout, and the fermentation is started by chewing the grain. . . . While maize beer is also made by semi-agriculturists east of the Andes, other tribes make an intoxicating beverage from cassava bread. Some of the semi-agriculturists do not know how to make beer at all, and it is likely that this art belonged originally to the maize-cultivating full-agriculturists of the Andes. . . . North of Mexico it seems that no alcoholic beverages were made in pre-Columbian times." (Curwen and Hatt: *Plough and Pasture: The Early History of Farming*).

Thus, in my opinion, the indirect evidence regarding our problem is in favor of bread being the first stimulant for agriculture; the evidence for the opposite solution is nil.

by PAUL C. MANGELSDORF

The idea that brewing antedates bread making is, of course, not new. At least one earlier student (Death) has called brewing the oldest of the arts and has even contended that it preceded the use of fire.

A fairly good case might be made for an earlier utilization of cereals for brewing than for bread-making, not because thirst was a stronger motive than hunger, but because the earliest grains available were more suitable for beer-making than for bread. The earliest cereals of the Near East—the wild wheats, *Triticum aegilopoides*, *T. thaoudar*, and *T. dicoccoides* and their cultivated counterparts, *T. monococcum* and *T. dicoccum*, and species of barley—are all characterized by the adherence of the glumes (husks or chaff) to the grains after the grains have been removed from the heads. Such glume-enclosed grains are, without additional processing, virtually useless for bread-making but all can be used for brewing and one, barley, is the cereal *par excellence* for this purpose.

But having made a case for the antiquity of brewing one finds several questions immediately arising. If these ancient cereals were not used for food, what did their gatherers or early growers feed on? Man cannot live on beer alone, and not too satisfactorily on beer and meat. And the addition of a few legumes, the wild peas and lentils of the Near East, would not have improved the situation appreciably. Additional carbohydrates were needed to balance the diet. No carbohydrate-furnishing plants were naturally available in this region, according to Vavilov, except the cereals. Did these Neolithic farmers forego the extraordinary food values of the cereals in favor of alcohol, for which they had no physiological need? Are we to believe that the foundations of West-

ern Civilization were laid by an ill-fed people living in a perpetual state of partial intoxication?

And for what purpose were the ovens used? Parching is not a part of the malting process; indeed, it is an infallible preventative of malting. To make an alcoholic beverage from cereals the starch must first be converted to sugars by enzymatic action. This may be done by one of three methods—germination, salivation or fermentation. In the first method the sprouting grain furnishes its own enzyme, diastase; in the second the primitive brewer supplies the enzyme ptyalin from his (or her) own saliva; in the third the enzyme is derived from the fermenting organism, usually a fungus. In all three methods the enzyme would be destroyed by the heat employed in parching. The beer-making of the Near East probably involved the first of the above methods, germination. Parching as an initial step would have completely prevented germination; applied following germination it would have inactivated the enzyme, diastase.

A test of the brewing theory might be provided by the prehistoric grains themselves, or by their impressions in pottery. Is there any tangible evidence of an early practice of sprouting grains? We have examined prehistoric grains from both Jarmo and Mattarah and find no evidence whatever of germination. Other collections might perhaps be profitably examined from this standpoint.

Perhaps the earliest beer was less sophisticated than modern beer and was made from ungerminated grains by fermenting the small amounts of sugar which occur naturally in cereals. Such a beverage would have had a very low alcoholic content and should perhaps appropriately be called not a beer but a gruel; not an alcoholic beverage but a food. The Hawaiian *poi* is such a food.

I suggest that the earliest cereals were used neither for brewing nor for baking, but as popped or parched grains. If cereals, in which the glumes adhere to the grains, are a regular and substantial part of the diet, it is necessary to remove the glumes in some way; otherwise the intake of indigestible cellulose is too high for the well-being of the human digestive tract. In all parts of the world people have learned to remove the adhering glumes of cereals by heat or fire. If the cereal is corneous and has a proper moisture content the grains, when exposed to heat, will explode or "pop" and escape completely from their enveloping glumes. In such cereals simple exposure to heat is a substitute for three modern processing operations: hulling, milling and baking. In America the earliest use of maize appears to have been for popping; popped kernels have been found in the remains of Bat Cave. In India certain varieties of rice are popped by stirring the kernels in hot sand, and many Indian villages still have a village popper who processes the rice by this primitive method and takes his toll of the product.

If the cereal is not corneous enough to pop, heating at high temperatures

will char the glumes allowing them to be rubbed off. The Chippewa Indians still employ this method in processing wild rice, first heating the rough rice, then removing the charred glumes by tramping the grains in a hollow log: a mortar with a human pestle. Clark described a practice in parts of Scotland of burning off the glumes of cereals by setting fire to the unthreshed heads.

If the heating is carried far enough to char the glumes the kernels undergo some degree of parching and are much more easily chewed or ground than the raw grain. We have experimented with grains of barley, einkorn and emmer which, in the raw state, are almost impossible to chew, but which, once their glumes have been charred and rubbed off and their grains slightly parched, are easily masticated and are quite palatable.

The next step, taken, perhaps, to meet the needs of the partly toothless members of the community, both old and young, was to render the grains still more easily masticated by grinding them coarsely and steeping the resulting meal in water. Körnicke states that the Greeks rarely used barley for bread. It was roasted and coarsely ground, originally perhaps for easier mastication, but later for preparing a gruel with water. The product, *alphita*, was the equivalent of daily bread for large segments of the population.

The practice of making gruel might easily have led to brewing and baking, since a gruel may undergo mild fermentation and since in both brewing and baking fermentation is an important factor. However, a possible intermediate step, the making of unleavened bread, should not be overlooked. Unleavened breads, made from all kinds of cereals, are used throughout the world. It is easy to imagine an historical sequence which involved (1) unleavened bread, (2) bread, leavened with yeasts, (3) beer, fermented with yeasts. The sequence (1), (3), (2) would not be completely improbable; the other four possible sequences are more difficult to imagine.

Since we are, in any case, dealing with largely untestable hypotheses may I suggest the following as a possible sequence of evolutionary steps:

1. Parched or popped cereals = cereal—heat.
2. Gruel = cereal—heat—grinding—water.
3. Unleavened bread = cereal—heat and/or grinding—water—heat.
4. Leavened bread = cereal—heat and/or grinding—water—yeast—heat.
5. Beer = cereal—sprouting—drying—grinding—water—yeast.

This hypothesis is admittedly somewhat prosaic and involves nothing more profound than the assumption that evolution, more often than not, proceeds from the simple to the complex. The hypothesis does have the merit of being supported by one bit of tangible evidence: archeological remains of well-parched grains. One further point; H. P. Lutz in *Viticulture and brewing in the ancient Orient*, described the making of beer from half-baked loaves of leavened bread. Here is one instance, at least, where bread is more than a precursor to beer; it is the principal ingredient.

by HUGH C. CUTLER

It is likely that the use of cereals for both bread and beer was important as a stimulus to selection and breeding of cereals in the early days of agriculture. As bread or beer, grains are more palatable than raw or parched grains or even as parched grain beverage or as porridge. It is likely that before agriculture was developed man already prepared unleavened bread, beer, and may even have occasionally produced leavened bread by accident. While it would be difficult to find evidence that alcoholic beverages were prepared before the invention of agriculture, the large number of wild growing materials used for beers, the simple methods involved, and the many groups of people who prepare beers support such a belief. Probably the best known wild materials used for beer in the New World are the pods of algarroba and mesquite, fruits of the pepper tree, saguaro cactus, murity palm, and wild pineapple. I wouldn't be surprised if some wild growing grasses were used in the Old World.

I doubt if one can consider the yeasts involved as the first domesticated plants because they are not consciously planted or maintained free from competition with other organisms. Many different kinds of yeasts were involved, as well as molds and bacteria. Early beer yeasts can be considered weeds, like the weed potatoes which grow on the margins of Bolivian Indian fields and are tolerated and the tubers harvested, although the Indian does not know how the weed potatoes arrived there.

I would expect to find some kind of vessels although these need not be large and might be of perishable materials like skin. Takana Indians of Bolivia use jars which may hold as little as one gallon, although the usual size is two or three gallons. I would not expect to find malting in the earliest beer making, although it is likely that parched grain was used early because it is more readily ground and beer might have developed from a parched grain beverage which fermented accidentally. It would be difficult to find early evidence for chewing grains for beer unless dental-plate-like masses of salivated grain (the "muko" of Bolivian Quechua Indians) are found.

by CARLETON S. COON

One of the earliest ways of preparing cereals for food has been omitted entirely, and another barely mentioned. The first is porridge. Mortars take precedence over grindstones archeologically. All over the Middle East people eat some kind of porridge as a family dish, easier to prepare than bread which, before the invention of metal, required a communal oven. Such ovens occur in the painted pottery stage, but apparently not before. In the Americas maize porridge is a familiar food throughout the agricultural regions. In South and Central America, maize porridge is converted into the well-known *chicha*, a form of beer. In the agricultural regions of the United States both porridges

and bread were prepared and eaten by the Indians, who made no beer. The relationship between beer and bread established in the Old World involves raising the bread with yeast, an agent of fermentation. You cannot raise maize dough unless it is mixed with wheat. So this functional relationship is lacking in the New World, but still some of its inhabitants made beer. The Indians of North America are culturally marginal to those of Central and South America, and may be presumed to follow the older pattern. If they had known how to produce an intoxicant it is hard to understand why they would have given it up voluntarily. Furthermore, the Indians of California were in a Mesolithic cultural stage, using wild seeds. They parched them and porridged them, but did not brew them. In my opinion the fermentation of beers, in the Americas, probably FOLLOWED bread as well as porridge making and parching.

In the Old World the latest surviving Neolithic culture of Middle Eastern origin was that of the Guanches of the Canary Islands, whom the Spaniards conquered in the fifteenth century. The Guanches raised wheat and barley. They parched these grains, after pounding or grinding them, and ate the resultant *goffio* as a staple. This is still used among the Moroccan Berbers as an emergency food and wartime ration. The Berbers also make porridge, and steamed coarsely ground wheat (*seksu*). In the mountains the tradition is that bread was not commonly baked until recent times.

The Guanches made no beer, or other intoxicant. The Berbers made wine right through the Muslim period, and some of them still make it. There is no beer made in North Africa today, except by Europeans, and no mention of it in the abundant historical record, at least in what I have read. If the Berbers had made beer I think we would have known it. In the Old World as in the New, the old peripheries are beerless. I do not think that the problem can be solved in its present stage since on one side the evidence is partial, on the other lacking. However, I believe that the bulk of the ethnological evidence runs against the theory of the priority of beer.

In summary: The archeological side, as represented by Braidwood, has some concrete evidence, but it is not enough. The botanical side, as represented by Sauer has no evidence which I am qualified to understand. The evidence of history and comparative ethnology favors Braidwood, but it is not conclusive.

by RALPH LINTON

I would take an intermediate position in the beer vs. bread controversy. From the types of cereal usage current among marginal groups such as Celts, Bantu and N. Chinese it would seem probable that the earliest usage of cereals was as cracked, toasted grits or as mush. Bread can be shown to be later at least for Greece, cf. Homeric poems. Leavened bread is, of course, later still and pretty certainly post beer. Hard to see how the use of yeast could have

been discovered otherwise. One suspects that beer itself may have resulted from accidental souring of a thin gruel made from sprouted grain which had been left standing in an open vessel. The fact that all the grain recovered is charred may well be due to the removal by decay of kernels not transformed to inert carbon. As regards implications of tools, rather finely ground flour is required for bread making. It is almost impossible to produce this with mortar and pestle and difficult to make it with rubbing stones unless grain is worked over repeatedly on finer and finer stones as in S. W. Indian "piki" bread. Most of European Neolithic querns and metates are relatively coarse, adapted to manufacture of grits rather than flour.

by JULIAN STEWARD

Since the distribution of grains and other cultivated crops in the New World is much wider than fermented beers, I would think that the beers came later but admit this is poor proof of the historical sequence.

by A. LEO OPPENHEIM

In my booklet "On Beer and Brewing Techniques in Ancient Mesopotamia" (published as a Supplement to the Journal of the American Oriental Society, 1950), I have given considerable attention to the complex technological developments leading to the brewing of barley-beer. The investigation of the pertinent Sumerian and Akkadian terminology has shown that the preparation of vegetable food stuffs (not only cereals), without the application of fire, developed into the manufacture of pulpy dishes (gruel)—made palatable by seasoning or by sour fermentation—and of "preserves" (such as malted barley, etc.). These techniques led then to the baking of barley-cakes as well as to the brewing of several types of beer-like beverages. Thus, the cuneiform text-material which covers more than three millennia with an abundance of references—often reflecting even the developments of the preceding pre-literate period—suggests the quest for food (preparation—as well as conservation techniques) has acted as a primal incentive for the utilization, selection and eventual domestication of the cereals.

May I, as an outsider, suggest here that the necessity to preserve the harvested crops constituted an important simulus yielding many novel techniques which decisively influenced the history of food-preparation. This holds especially true for fruits (dates, grapes, apples, berries, etc.) which show a development via "preserve" to alcoholic beverage which technologically parallels that from grain to beer and might well have served as a prototype for the latter because the utilization of such fruits necessarily must have preceded that of grains. Again, the cuneiform material offers us in its technical terms, records,

and lists, an impressive amount of pertinent information which will shed, if adequately studied, light on technological developments which we otherwise would have to reconstruct solely on the basis of tenuous hypotheses.

This observation induces me to make use of the rare opportunity given to an Assyriologist to reach the scholars beyond the narrow boundaries of his discipline. I would like to bring to the attention of the anthropologists the fact that the early "literate" civilizations, especially Mesopotamia, through the imperishable nature of its writing-material, have preserved a wealth of important anthropological information which will come to light and to life only if the two disciplines cooperate. Neither the Assyriologist with anthropological "interests" nor the anthropologist who attempts to gather some "information" from the so-called handbooks can achieve valid results. A planned interdisciplinary approach, however, is bound to yield a rich harvest of important information, and to act as a mutual stimulus.

Rejoinder

First it seems that I should clear up the implications of the first paragraph of my original query. By "domestication (in a meaningful sense)," I mean the establishment of a level of food-production of such efficiency that a primary farming-village community may result. I cannot test the various botanically based claims of the Sauers (*père et fils*), Schiemann, Anderson, *et al.*, for "domestication" in further Asia, but I do believe the archeological evidence points to "nuclear" Western Asia (the hilly flanks of the fertile crescent) as the region in which *effective* food-production and the appearance of the village community first took place.

There was very probably an earlier phase or two of established village life, before that indicated by the Jarmo materials. Jarmo simply happens to exhibit what are apparently the earliest comprehensible remains of a village establishment which we now have in hand; the basal levels of Jericho may prove to be equivalently early (C^{14} samples from Miss Kenyon's competent new exposures at Jericho are now en route to Chicago). Karim Shahir may belong to one of these postulated pre-Jarmo village phases, but its "villageness" is not yet very comprehensible. If the earliest Near Eastern beer was brewed from germinated grain malt as Mangelsdorf thinks probable, and as Oppenheim's study (*op. cit.*, pp. 13–16) indicates was customary in later times, then we have no evidence of the process at Jarmo. Neither Helback nor Mangelsdorf, who have examined the Jarmo grain samples, found traces of germination.

Most of the respondents suggest the original use of the cereals to have been that of a food, prepared as a gruel. In connection with Sauer's point about the shattering of wild grain when reaped, I know of at least one small

village in northwest Syria where the standard Near Eastern *burghul* (wheat grains which are first boiled, then sun-dried, cracked into grits, and finally served as a hot mush-like dish) is prepared from wheat which is not quite ripe. Another case of the use of green kernels is *shawi*, in which full-sized but still somewhat green wheat is parched (it does not fully pop) and eaten immediately as a sort of delicacy on festive occasions—much in the way we use pop-corn. This would all fit in with the notion of gruel, and even with Mangelsdorf's pre-gruel popping stage. Such could explain the ovens with hard-baked flat floors at Jarmo, and also the quantity of overparched (accidentally carbonized) grain.

THIS ISSUE, AND OTHERS

(*Continued from page 514*)

American Foreign Service (Casablanca, Calcutta, Washington), and Harvard again (1949–50 Social Relations, especially anthropology). "Since then I have been engaged in independent research in a field generally known as the philosophy of history, but which may perhaps be more accurately described as the comparative study of the course of development of the higher cultures. I am now preparing a book on this subject for publication."

Many readers write that they like our new departures—Echoes, the Symposia, Conferences. We intend to continue and develop them. A forthcoming Wenner-Gren Supper Conference at Harvard will bring together old and new world prehistorians in a discussion on method, which will be duly reported. Queries for further Symposia, to follow Words and Beer, are wanted. Also Echoes.

We expect to make good our promise of another memoir this year, if the printers grind fast. It will be a good fat book on Chinese thought which will also be published commercially. It will come free with the December *Anthropologist* or in any event with the February issue.

Our next issue will also carry the paper chosen by the Central States Anthropologist Society as the best given at its Annual Meeting this year in Urbana. Other Societies may wish to announce similar competitions; and if the editors agree that the best is good enough, we promise speedy publication and an appropriate footnote. The CSAS voted to honor its member the more—and support our expanding publication program—by paying from its treasury the cost of the extra pages.

Ethnography, Cultural and Social Anthropology

By ROBERT H. LOWIE

THE discussion by Professors Murdock and Firth, Professor Fortes's contribution to the debate, Professor Radcliffe-Brown's illuminating letter in a recent issue of this journal, and a number of other statements by American and British colleagues (Murdock 1951; Firth 1951; Radcliffe-Brown 1952; Fortes 1953; Evans-Pritchard 1951) stimulate reflections on cultural and social anthropology. In the present, wholly uncontroversial article I shall first define the aims of cultural anthropology as I understand them and shall then inquire into the relations of that discipline with social anthropology as defined by British scholars.

I

Whatever differences may divide cultural from social anthropologists, they are hardly greater than those which divide self-styled cultural anthropologists. Indeed, I should say that many of us feel incomparably closer to the English anthropologists referred to above than, say, to Goldenweiser in his later phases.

A concrete example will illustrate the issue. In one of his books (Goldenweiser 1922) this writer devotes a chapter to the Baganda, relying as he was bound to do on Roscoe's well-known work. He tells us that "maize is perhaps the principal staple food, but plantain trees are also cultivated on a large scale." Now the primary source (Roscoe 1911: 5, 432) states in unmistakable terms that *plantains* "furnish their staple food," whereas maize "was never grown in any quantity . . . ; no one called the two or three cobs which he ate a meal." Of course, anyone is liable to factual inaccuracies, but what is involved here is something deeper. Goldenweiser writes about these matters as might any layman—not as a student of culture. He does not explain to his reader that maize is a species introduced into Uganda in recent times. He shows no inkling of the problems connected with the occurrence and distribution of East African plantains, indigenous and otherwise. Yet in his several publications there is no end of references to culture—to "psychology and culture," to "culture and environment," to "the theoretical categories and cultural reality," and so forth.

It would be convenient to have an English term designating the kind of cultural anthropology of which some of us—say, Kroeber and myself—are votaries, a term corresponding to Klemm's "*allgemeine Culturwissenschaft*." For the purpose intended, "cultural history" will not do since it one-sidedly circumscribes the discipline; and notwithstanding the analogy of "sociology," many of us recoil from deliberately perpetrating a twin monstrosity such as "culturology." However, Professor Radcliffe-Brown informs us that since 1909 British colleagues have used "ethnography" to denote "descriptive accounts of

non-literate peoples." The accounts, I assume, relate to culture. I accept the definition with an extension of meaning. Greek "ethnos" assuredly did not apply solely to savages or barbarians, and non-literacy is obviously not a sharply defined concept. Ghegs are not literate in the sense that Scandinavians are; and the Maya present a borderline case. Further, it is not clear that in principle a report on the social life of ancient Egypt, modern China, or seventeenth-century England (witness the famous third chapter of Macaulay's *History*) differs from a report on the Kariera, the Yurok, or the Maori. Accordingly, I suggest that the term "ethnography" cover all cultures, past and present—thus becoming an equivalent for a "general science of culture." In accordance with my conceptions of science, which are those of Ernst Mach and G. R. Kirchhoff, I define the ideal aim of ethnography as the *complete* description of all cultural phenomena everywhere and at all periods.

Obviously one individual cannot thoroughly know more than a limited number of cultures; and with growing specialization he cannot even completely master all the aspects of a single culture. Nowadays the same person is not likely to describe social structure with the thoroughness of a Firth or Fortes and at the same time penetrate the intricacies of textile work with the devotion of an O'Neale or a Bühler. This has long been recognized—witness the division of labor among participants of the Cambridge Expedition to Torres Straits, Haddon taking over technology, Rivers social organization, and so forth. Yet, as a student I heard Haddon expound Rivers's genealogical technique for studying kinship terminology. There lies the crucial point: the true ethnographer may not be competent to advance knowledge along all lines within his science, but he takes an interest in what is done by fellow-ethnographers who happen to be drawn to other departments of culture. Characteristically he despises no item of culture as intrinsically negligible. However trivial it may appear at first blush, its correlates may be of the highest significance. The avoidance of parents-in-law doubtless struck many as merely "a quaint and somewhat comic custom" of primitive etiquette, but for Tylor it inaugurated a most stimulating line of research.

What distinguishes my ethnographer from the cultural anthropologist à la Goldenweiser is that the ethnographer uses terms not as counters in a metaphysical game, but to circumscribe an empirical content which he hopes to enlarge. If he deals with "plantains," he determines their place in tribal economy, the method of cultivation, the genetic relationship of the form planted with indigenous or alien wild forms of *Musa*, and so forth. Without such information he does not feel that he understands this particular facet of Baganda culture; with all the information available about the global distribution of *Musa*, the historical connections of its varieties, the uses to which they are put, etc., he understands what *can* be understood at the present time.

As the illustration indicates, the ethnographer cannot always obtain the

information he craves unaided by other disciplines; but such recourse to outside aid does not involve surrender of his autonomy. It is he who frames the problems, and there is never a question of merging the distinctively cultural phenomena in botanical or other terms.

It may be well to exemplify by a series of specific cases.

In Heizer's recent paper on piscicides (1952) there is, first of all, a definition of how the phenomenon under discussion is globally distributed. The author eliminates sham problems by supplementing his findings with a statement concerning the distribution of narcotic species. Obviously where such are lacking they cannot be utilized. Heizer further inquires whether natives always avail themselves of the opportunity to drug fish where nature does provide it, to what extent they deliberately cultivate narcotic plants for piscicidal objectives. He realizes that the data collectively impinge on no less a problem than the logical or prelogical mentality of man. Certainly there still remains a great deal to be learned about piscicides; but Heizer has appreciably added to our comprehension of the problem and worked towards that complete determination of relevant facts which constitutes the aim of a student interested in the realities of culture, not in an abstraction divorced from these realities.

Corresponding reflections hold for animal domestication. To take the much-mooted problem of the reindeer, any new zoological or geographical data may help towards a better view of the cultural problems. It is important to know that the species flourishes in the taiga as well as in the tundra; important to note the physical varieties that permit or preclude mounting the animal. Highly significant, too, is the discovery that—contrary to oft-repeated statements—reindeer, if kept in small numbers, are "thoroughly domesticated and tame as are the horses kept in stables." It alters the conception of the species as inherently migratory (Jochelson 1926: 361–367).

As the complete ethnographic picture may require the help of botany or zoology or metallurgy, so it may require that of psychology. The phobia of that science on the part of some cultural anthropologists and sociologists is historically intelligible, but logically indefensible. It is a survival of the period, long past, when students of society or culture had to insist that their subject matter demanded a special branch of learning. But what is at stake nowadays is something quite different. As some cultural phenomena cannot be grasped without the geographer's, botanist's, or metallurgist's help, so others require the light of psychology. Child behavior, e.g., as traditionally determined, is unquestionably a part of a given society's culture, hence Margaret Mead (1933) is wholly right in demanding that the old ethnographer's random observation of this field be superseded by systematic observation through approved psychological techniques. Such procedure in no way involves a subordination of ethnography to another discipline of supposedly higher hierarchical status.

To renounce on principle whatever illumination psychology may offer is to

renounce complete comprehension of many cultural phenomena. If "Arctic hysteria" is characteristic of groups widely separated, its occurrence evokes an urge for a fuller grasp of what Tylor would call its "adhesions." Psychology may not yet offer anything like a definitive interpretation, but even the tentative suggestion that the phenomenon goes with "a subservient or submerged social position," that it is more commonly associated with women, is not without value (Aberle 1952). As to shamanism, the term is a meaningless counter unless we ascertain to the fullest extent possible the mental state of the shaman and the responses of his group. Surely it is not a matter of indifference to the ethnographer whether, as Jochelson and Bogoras suggest, the shaman is a neurotic, if not a madman and invert, or whether according to investigators elsewhere he may be quite normal (Jochelson 1924: 199 f.; Lindgren 1935: 222 et seq.). Correspondingly, it is part of the ethnographic phenomenon whether the shamanizing practitioner is a humbug, whether he wholly loses consciousness, or approaches that state (Seligmann 1911: 133 f.).

Of course, the "complex whole" defined by Tylor as constituting culture embraces many phenomena not dealt with by any natural science. With reference to them, the ethnographer may either utilize techniques perfected by the humanities or devise new avenues of approach. As noted, one criterion of the ethnographer is his willingness to attack the apparently trivial, recognizing that like Tennyson's "flower in the crannied wall" it potentially sheds light on the cultural universe. Tylor (1896) did not disdain to survey lot-games, thereby arriving at significant conclusions (whether right or wrong) on Asiatic and American connections and at equally significant methodological considerations.

In order to preclude misunderstanding of my position, I turn from Tylor to Culin (1907). Probably there is not for any part of the globe a comparable mass of information on aboriginal amusements. However, it is raw material with a minimum of interpretation and even of serviceable arrangement. That is to say, though the description of the several games is often conspicuously full, the relationships of the facts to one another remain unilluminated. The monograph aptly illustrates the truism that a mere accumulation of data yields no significant conclusion. What is, however, often ignored in similar contexts is that, though men like Culin fall far short of that *complete* description which involves insight into correlations, their faithful gathering of facts makes it possible for others to extract meaning that they themselves failed to gain.

To take one other department of culture, ethnographers concerned with aboriginal literature have not merely collected tales and songs, but have applied to them the methods current among literary critics in higher civilizations. Boas (1916; 1927: 299 et seq.) investigates what aspects of a tribal culture are reflected in tales, the rhythmic repetition in aboriginal narratives, the metaphorical expressions in their poetry. Miss Beckwith (1919: 296–331;

1951) besides shedding light on the correlations of Polynesian literature with belief and social organization, has enlarged on the specifically poetic attributes of Hawaiian poetry. Radin (1915; 1948; 1949) has for forty years discussed problems of literary style and the relations of the author-raconteur to the traditional body of literature. Reichard (1947) compares the styles of neighboring tribes in the northwestern United States; Shimkin (1947) points out the comparative literary richness of an originally Basin people with a meager technology and the dependence of style on individual personality. Comparable researches have been credited to Russian folklorists, but the only European study I can recall having a similar orientation is Thurnwald's discussion of Papuan songs (1936).

Let me repeat that at present no one scholar can be expected to master to an equal degree all the subdivisions of culture. Even before the present degree of specialization it was impossible for one man to add with uniform success to the several departments of the total field. Over half of Tylor's *Primitive Culture* was devoted to religion and myth; Boas probably displayed most enthusiasm for decorative art and folk-literature; Haddon is known above all for his technological studies and his discussions of art. But these men were true ethnographers on two counts: they did concern themselves with the whole or at least the major part of the total range of cultural phenomena; and where they specialized they delved deep. *They were interested in the realities of culture.*

II

From the point of view here assumed there is no difficulty in fitting contemporary British social anthropologists into the picture. I accept them as true ethnographers, as scholars who at their best have dealt admirably with an important subdivision of culture. I recognize that some of them moreover show a commendable concern with other aspects of culture as well. I admit that it is profitable to view the culture of a people with social structure as a starting-point. What I must unequivocally reject, however, is Dr. Fortes's contention that social structure "is not an aspect of culture but the entire culture of a given people handled in a special frame of theory." In my opinion the social structure of a people by Tylor's definition is one aspect of their culture. For example, matriliny is not determined by the biological heredity of a group nor by the biological idiosyncrasies of given members of a group, but is part of the social heritage of the constituent individuals. Further, pending proof to the contrary, I cannot conceive the *entire* culture of a given people being subsumed under the head of social structure. Certainly it is possible to link crafts with social groups or to connect forms of religious faith, say ancestor worship, with segments of the society, and to do so is eminently worth while. But to do so in no way eliminates the need for studying the crafts—men's technological processes or the content of the beliefs. The cultural residue that

remains after approaching "the entire culture" from Dr. Fortes's point of view strikes me as appallingly large.

III

Finally I turn to the familiar dichotomy: historians versus generalizers. Undeniably it is vital in some contexts, but I cannot admit that it serves to distinguish my "cultural anthropologist," i.e. ethnographer, from the social anthropologists. The ethnographer, attempting to fathom the infinitely varied reality of culture, must cope with many problems that in themselves neither lead to historical reconstruction nor to the formulation of laws.

In his discussion of a Mohave epic Kroeber (1951) has to broach the question whether the incidents narrated as historical by the native informant had actually occurred; but this inquiry has nothing to do with tracing the antecedents of the tale, its possible diffusion, for instance, from an outside center. Of course, the historicity of the episodes has a bearing on the past of the Mohave people. But it bears no less on a generalization explicitly formulated by Kroeber: The memory of a group in the status of the Mohave cannot be trusted beyond a century or two. In the same paper Kroeber indulges in many comparisons of his Indian tradition—with the Homeric and Hindu epics, with Biblical stories, with the Book of Mormon. His intention is not to derive the Mohave narrative from any of these nor to demonstrate a law. He is trying to define the individual phenomenon with which he is dealing. When he aligns Mohave with Homeric use of proper names (1951: 132), the comparison merely tries to bring home the nature of the aesthetic effect achieved by the Indian. What Kroeber envisages here, as in most parts of his anthropological work is, to use his own phrase, "descriptive integration"; he has called this aim "historical," but it is historical in a novel sense and one quite distinct from that given to the term by social anthropologists (1935: 545 f.).

What Kroeber calls "descriptive integration" evidently comes close to my "complete description." Such complete description implies cognizance of correlated phenomena and thereby precludes erroneous inferences due to the currency of the same labels. The Bronze Age of Peru is not the equivalent of the Bronze Age in the Old World. As Nordenskiöld remarks, it "does not mark the adoption of a new civilization but only an amplification of the copper culture in Peru" (1931: 40).

Complete description involves a *global* survey of correlations because only such a global survey guarantees accurate definition of the cultural phenomenon under discussion in relation to its real or apparent equivalents elsewhere. When, however, these correlations are lifted from their respective contexts and found to coincide, when diffusion is eliminated or rendered improbable in the cases investigated, the ethnographer achieves precisely the kind of

generalizations—functional relationships between descriptively separable elements—that a sound social anthropology aspires to (Eggan 1950: 323). The ethnographer would fall short of his own objectives if he failed to arrive at such generalizations or renounced them on principle.

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Culture and the Causes of Culture

By PHILIP H. BAGBY

IN A recent issue of this journal Coulborn (1952) has reopened the question of the causes of culture, which has been discussed in these pages sporadically—and one might say inconclusively—during the last decade. As one rereads these articles (Bidney 1944, 1946; Kroeber 1948a., 1951; Strong 1948; Radcliffe-Brown 1949), it is hard not to feel as if one were grappling with phantoms which slip away as soon as one tries to grasp them. It seems likely that, as so often happens, the main difficulty lies, not in the nature of the problem itself, but in the vague and loose way in which we customarily make use of words. Perhaps if we could define such basic concepts as "level," "culture" and "cause" a little more precisely than we are accustomed to do, we would find that at least some of our difficulties had been removed and that the problem had been clarified, if not solved.

Science is by definition orderly knowledge and each particular science must therefore deal with some particular type of order which man has discovered in the universe which surrounds him. It is customary to say that a science deals with "regularities" or "correlations" of phenomena and these terms are only another way of referring to the orderly aspects of our experience. Such regularities may be concurrent or successive, synchronic or diachronic; that is to say, two or more phenomena may appear together (roughly) at the same time or in a regular temporal sequence. Moreover, such regularities, though often directly observable, are nevertheless in a sense abstracted from the total flux of phenomena. The objects of ordinary discourse, for instance: tables, chairs, dogs, cats, human beings and so on, are all abstractions of this kind; they are concurrent regularities which happen also to be continuous in space and time. We take a number of phenomena which regularly (and continuously) appear together—a flat hard surface resting on tall, thin legs, for instance—and consider them apart from other surrounding or interpenetrating phenomena which change. Because these groups of phenomena are continuous in space and time, because they reflect light and are resistant to the touch, we think of them as being in some sense concrete; they are nonetheless abstractions from the total experience which alone deserves to be called fully concrete.

Some of the sciences—biology and geology, for instance—deal in large measure with concurrent regularities similar to the objects of ordinary discourse; for this reason they tend to be largely descriptive and classificatory. But in the more refined of the physical sciences, which deal only with certain measurable aspects of phenomena, we may have concurrent regularities which are discontinuous in space such as the phenomena of gravitation. All the sciences deal with successive regularities if they can find any; we have the

laws of growth in biology and the laws of mechanics in physics. Occasionally we may even find successive regularities which are discontinuous in time; nuclear radiation is an excellent example. It must be stressed, however, that all such regularities are equally abstract and for that matter equally concrete in any epistemological sense.

Here and there in the sciences we may find objects which are not directly observable—electrons in physics, for instance, or genes in biology. These are generally called hypothetical constructs; we cannot really know whether they exist or not but they are extremely useful as a means of referring to or explaining broad sets of regularities which *are* observable. It is important to note however that they do not necessarily represent any further degree of abstraction.

The type or types (class or classes in the terminology of modern logic) of abstraction with which each science deals is usually called its subject-matter and these subject-matters may be related to each other in a number of different ways, as a cursory inspection of the various sciences will show. One of the sciences may include another as biology includes zoology, or it may exclude another while remaining a branch of the more general science, as zoology excludes botany but remains a branch of biology. It would be more exact to say that it is the subject-matters of these various sciences which include or exclude one another rather than the sciences themselves, but as long as we realize this there is no harm in using the shorter and easier form of speech. One science may include or exclude another then, but if precisely defined, one science should not overlap another. That this is not always possible is shown by the existence of various borderline disciplines such as physical chemistry or biochemistry. Sometimes one science will deal with the gross aspects of certain phenomena while another will deal with smaller aspects, and the first is by no means always reducible to the second. More often than we should like, the subject-matters of two sciences are simply incommensurable with one another; the two classes of abstractions are defined so differently that no relationship can be established between them.

The concept of "level" seems to represent an attempt to fit this fact of incommensurability into a general world picture. Although similar notions may be found in Aristotle—his theory of the four kinds of souls, for instance—their reappearance in the nineteenth century appears to be connected with the evolutionary ideas prevailing at that time. What Comte was trying to do was to fit the subject-matters of the various sciences into certain larger classes which would be fully homogeneous internally, though incommensurable with each other. The sciences were then thought to be related to each other genetically, a more complex science rising from a simpler one by a process inherent in human thought. From a practical scientific standpoint, such inherent or necessary dialectical processes—laws of thought discovered by pure thought

—can only be considered as highly suspect. Later thinkers supposed that this evolutionary process took place not only in the realm of thought, but also in actuality, the world of biology having evolved out of the world of physics and so on.

This latter theory is suggestive and may have some basis in fact, but it cannot be said to have been proved or to have solved the question of incommensurability as it sets out to do. For one thing, what we are talking about are *abstractions* and it seems to be inherent in the process of abstracting that we can make diverse and incommensurable abstractions from the same set of phenomena. Moreover, even within the levels which have been proposed there has been a stubborn persistence of incommensurability. Einstein, for instance, has tried and failed to establish any satisfactory relationship between the large-scale phenomena with which the theory of relativity deals and the subject-matters of the other branches of physics. In psychology too there seems to be an inherent gap between its two main branches, the study of individual personality and the study of human nature in general, which Allport (1938: 22), following Windelband, has distinguished as the "idiographic" and the "nomothetic" branches of that science. If we were to venture into these treacherous metaphysical quicksands, we might guess that the coexistence of diverse and irreducible types of order is inherent in the structure of the universe, and that divergent types of order may have evolved separately much in the same way as there seem to have been divergent lines of biological and cultural evolution. But this can only be a guess at best.

What does seem to be useful and worth retaining in the concept of "level" is the notion that some classes of abstractions are commensurable and others not. As we shall show later, culture, the subject-matter of cultural anthropology is commensurable with the subject-matters of certain other sciences but is *not* commensurable with still others. Before demonstrating this, however, we shall need a fairly tight and homogeneous definition of culture.

As for the term "level," we might conceivably retain it to refer to any of the more general classes of abstractions which include all mutually commensurable abstractions of a single kind. But the word has come to be used so loosely to cover all sorts of classes (or classes of classes) of abstractions that confusion would inevitably follow any attempt to restrict its meaning. We had best take "level" to mean any class of abstractions whatsoever, while we can refer to the more general classes of all mutually commensurable abstractions as "homogeneous fields of enquiry."

Our next task then will be to construct a precise and homogeneous definition of culture. We do not need to review all the literature on the subject since we are not trying (as some authors have done) to construct a synthetic definition which would include or imply all that is believed about culture. On the contrary, what we need is the smallest possible number of indices which

will enable us to recognize an abstraction as belonging or not belonging to the particular class of abstractions we call "culture."

Perhaps the broadest description of culture to be found in the literature is the phrase: "what men think, what they do and what they make." This description can hardly be called a definition; it is far from homogeneous. In fact it includes three quite different classes of objects or abstractions: mental events, behavior and material objects. Let us see if we cannot reduce them to a single class.

As far as material objects are concerned the task is easy. Most anthropologists would agree that what they are interested in is not the material objects themselves, but how they are altered and used by human beings, that is, the behavior of human beings towards them. Material objects are only useful to the anthropologist or the archeologist as evidence of how human beings behaved in the past; they tell us about behavior which can no longer be directly observed. Such objects also give some evidence as to the ideas and feelings, the mental events of our forebears. But in either case, material objects can be eliminated from our definition.

We are left then with the two other categories, mental events and behavior; it will not prove so easy to reconcile them. Most anthropologists would wish to keep both categories and would maintain that an understanding of the contrast between idea and action, between theory and practice, is essential to the interpretation of any particular aspect of culture. Nevertheless, for our present purposes we need a homogeneous definition. Let us see if we cannot obtain it without losing this important distinction.

Mental events are not usually, of course, directly observable; their existence is customarily deduced from observation of behavior. We can observe them directly in ourselves by that curious process known as introspection and we are undoubtedly justified in supposing that they also exist in others. For the purposes of this article, however, we can assume a behavioristic position and take such words as "ideas," "feelings" et. cetera as only a shorthand way of referring to certain configurations of behavior, the same configurations which in another context we would regard as constituting the evidence for the existence of certain mental events. In this way we shall have succeeded in reducing our three categories to one. The distinction between theory and practice we would now regard as a difference between verbal behavior and other behavior, a point of view which is enshrined in such popular expressions as "Do what I say and not what I do."

Our procedure will seem all the more justified if we consider certain recent developments in psychology. The psychoanalysts and the dynamic psychologists in fact have taught us that all behavior must be regarded as evidence for some sort of mental event, conscious or unconscious. The contrast between theory and practice they would regard not as a contrast between mental event

and behavior, but as a contrast between conscious and unconscious mental events or between *superego* and *id*. We might of course follow their lead and define culture as being entirely composed of mental events. But then we should be including, without any overweening necessity, a large number of inferential constructs, the so-called unconscious mental events. We shall do better to stick to behavior which we can observe, and leave mental events out of our considerations entirely.

We have thus reduced our three categories to one. Culture is behavior. But surely not any kind of behavior. If so, we should be unable to distinguish the respective subject-matters of cultural anthropology and behavioristic psychology. Culture must be some particular kind of behavior or some particular aspect of behavior. To discover what kind we might turn to one of the oldest and still widely quoted definitions of culture. Tylor uses the expression: "acquired by man as a member of society." This lapidary phrase seems to have fused two distinct criteria: first, that the kind of behavior we call culture has certain origins or causes; second, that it is similar to the behavior of other members of the society of which an individual happens to be a member. This first criterion rather begs the question as to the causes of culture (as do certain more recent definitions in terms of learning theory) and had therefore best be eliminated. We are left with the second criterion: culture is behavior similar to the behavior of other members of some particular society.

Yet there seems to be something wrong with this definition. Like so many other so-called definitions it points to the general area of enquiry rather than to the specific type of order in which we are interested. Surely it is not the particular acts of behavior as such that interest us, it is their similarity. Having abstracted from the flux the configurations we call behavior, we must now go further and abstract from behavior the configurations we call similarities. We can now make our provisional definition of culture read as follows: "Culture is similarities of behavior in the members of a particular society." We are not only interested, however, when all members of a society behave alike; the anthropologist is also concerned if they behave differently but in a regular fashion. If the men in a society all wear trousers and the women all wear skirts, that is surely culture. It is culture too if the king wears a crown and his subjects do not. To cover these and similar instances, we had best use the word "regularities" instead of "similarities," the latter being included in the former as a limiting case. Our provisional definition would then be amended to read: "Culture is regularities of behavior in the members of a particular society." (I seem to recall having seen a similar definition in one of the works of Margaret Mead, but have been unable to find the precise passage.)

The difference between "regular behavior" and "regularities of behavior" is a subtle and yet an extremely important one which has often been overlooked or slurred in anthropological literature. We often speak loosely as if

some single act of behavior—Dick's drinking coffee at 8:15 on the morning of March 22nd, for instance—were culture, but it is really the similarity of this act (or its regular relationship) to the acts of other members of the same society that concerns the anthropologist. The individual act indeed cannot be the subject of any science; it does not of itself exhibit any order or regularity which science can study. Standing alone it belongs to the domain of history or perhaps of biography. Cultural anthropology, however, insofar as it aspires to be a science, must deal with some sort of regularity; our definition of culture makes clear that it does deal with certain specific regularities and that its claim to be a science is therefore fully merited.

We now have a precise and homogeneous definition of culture. But our difficulties are not yet at an end. We are still in considerable danger of having our subject-matter overlap the subject-matters of several other sciences. Physiology for instance deals not only with regularities of structure in human bodies, but also with regularities of function (that is, of behavior) insofar as they seem to be clearly inherent in physiological structure. Eating, for instance, is behavior common to all members of all societies and under our definition this regularity as it occurs in each particular society would be a part of the culture of that society. But anthropologists commonly prefer to regard the mere fact that all members of a society are accustomed to consume nutritious substances as a physiological matter; it is the particular ways in which this universal human tendency is modified in particular societies that interest them. The nomothetic branch of psychology also purports to deal with regularities of behavior in the members of all societies; its subject-matter is usually known as "human nature." In practice, of course, Western psychologists have tended more often than not to discover regularities which only hold for members of the Western "higher culture," but we may grant that they have a legitimate field of inquiry which must be distinguished from that of the cultural anthropologists.

We could exclude the subject-matters of these two branches of science, the functional branch of physiology and the nomothetic branch of psychology, by amending our definition to read: "Culture is regularities of behavior among the members of particular societies but not among members of all societies." But if we were to do so, we would find ourselves having also excluded, in the excess of our zeal, such cultural universals as language. The fact that all men speak some language and that such languages all have phonemic and morphemic structures would now become a purely psychological or physiological fact. Perhaps a better method of achieving our purpose—and one more in conformity with common practice—would be to exclude from culture such regularities of behavior as are clearly hereditary in origin. This would have the additional advantage of excluding not only such regularities as psychological functioning and human nature, but also regularities in the behavior of

races. None of these latter have as yet been proven to exist, but on the other hand it is impossible to prove the contrary, that races have no psychological peculiarities whatsoever. While the abuse of unproved racial theories for sinister political ends cannot be denounced too severely, nevertheless racial psychology must always remain at least a potential form of science and therefore its domain must also be distinguished from that of anthropology.

The realm of culture, then, is constituted by regularities (other than those which are clearly hereditary) in the behavior of members of particular societies, a society being here defined simply as a group of people who interact with one another more than they do with outsiders (see Kluckhohn 1950: 31). Much of the ground covered in reaching this definition is of course familiar to all anthropologists and its value lies not in any novel features but in its precision and homogeneity. These virtues should enable us to give much clearer and more logical consideration to the question of causes.

Before going on we should stress the fact that the regularities which the anthropologist studies are just as real as the subjects of the other sciences or indeed as the objects of ordinary discourse. If we had defined culture in terms of some such hypothetical construct as values we should be on shaky ground, but we have defined it in terms of *behavior* and no one doubts the reality of behavior. It is true that cultural regularities are not continuous in space or even always continuous in time like tables and chairs, and it is only on the rare occasion—watching the crowd at a football game or looking down on a landscape from an airplane—that we can embrace some aspect of culture or its effects in a single sweeping glance; but culture is nevertheless an observable aspect of the phenomenal universe and therefore perfectly real. Insofar as it is divisible into separate items, these items are real entities. Of course, it is true that culture has no substance; it is composed of forms and nothing more. But in the view of almost all modern philosophers, idealists as well as positivists, this is equally true of everything else. There was no need for Kroeber to make his famous recantation (1948a.: 407–408); to object to the reification of culture is to object to talking about it at all.

We need not claim that the definition we have now arrived at will turn out to be the best in every context. It does have, however, the advantage of clarifying the relationship between cultural anthropology and certain neighboring sciences such as sociology, cultural history, and cultural psychology. If we try to construct a definition of the subject-matter of sociology along the same lines as our definition of culture, we find that sociology deals with regularities (again excluding clearly hereditary ones) in the behavior of members of the same society *towards one another*. An item of behavior directed towards another human being is what the sociologists call an "act" or a "social action"; the reciprocal patterning of such acts in roles and institutions constitutes "social relations," "social organization," "social systems," or even "society,"

where we mean by "society" more than a simple aggregate of interacting persons. The word "society," it should be noted, appears to have two different senses which are often confused: (1) an organized (or regularly interacting) group of persons, and (2) the organization (or regularities of interaction) of a group of persons. Insofar as sociology is a science, it deals with society in the second sense and not the first. The subject-matter of sociology then is a part of culture, and sociology, in a strict hierarchy of sciences, would be a branch of cultural anthropology. No doubt social relations form the largest and in some ways the most important part of culture; almost every instance of human behavior contains some reference, direct or indirect, to other human beings. Even hermits, it is said, constantly seek the approval of their fellows.

Historically, of course, sociology has developed as a comparative study of social institutions in our own or closely related societies, while the anthropologist, in dealing with small non-literate societies has had to study all the branches of culture, including social institutions, at the same time. For these historical reasons each of the two sciences has developed its own vocabulary and its own techniques; it is often difficult to coordinate or reconcile them. But this does not mean that society and culture constitute two different levels of abstraction or two different realms of existence, as has often been supposed. On the contrary, social organization is a part of culture and nothing more. Just as economics, for all its elaborate and refined methodology, is merely a special branch of sociology, so sociology in turn must be subsumed under cultural anthropology.

The relationship between history and anthropology, however, is not quite so simple. Indeed, much of what is called history is not science at all. Historians deal primarily with individual human events—important events as they have occurred in the past in literate societies. They seek, first of all, to describe these events in their full individuality, without trying to find any regularity or order in them. Inevitably, of course, being subject to the general human love of regularity, historians do tend to suggest or imply the existence of certain patterns of regular change, but they have rarely tried to establish the existence of these patterns on any firm rational basis. And when they have tried they have usually failed. In the last century or so, however, contemporary with the rise of sociology and anthropology, there have appeared new kinds of history, cultural or social, which deal with regularities in the behavior of members of particular literate societies in the past. Since these new branches of history deal with regularities, they may properly be called sciences; and since they deal with cultural or social regularities, they are all clearly branches of cultural anthropology. History is a science, then, only insofar as its domain overlaps the domain of anthropology.

Cultural anthropology, then, includes sociology and the cultural and social branches of history; we shall see that its domain also overlaps the domain of

psychology. The psychologist studies primarily the regularities of behavior (and thought as evidenced by behavior) in a single human being. These regularities are conceptualized as habits, mannerisms, personality traits, et. cetera. The sum of the regularities exhibited by an individual is known as his personality, and the whole field is usually known as that of personality studies. We may think of each man or woman as being subject to his own laws (if we wish to borrow that much-abused term from the physical sciences) which apply only within the spatial and temporal limits of his behavior. But some of these regularities are reproduced (under a given set of circumstances) in all human beings or at least in all of a particular sex and age, and here the psychologist is able to formulate more general laws or regularities, which are thought of as being somehow hereditary in origin, as forming part of human nature. The psychologist would no doubt willingly exclude from his domain such features of behavior as are clearly derived by imitation, or reaction, from the behavior of others. He would prefer to retain only those which are clearly innate, but often he finds it impossible to make a clear distinction. Moreover, as a matter of practice the psychologist finds it most convenient to investigate the behavior of members of his own society; thus it is easy for him to mistake cultural regularities for general laws of human nature. It will in all likelihood prove impossible ever to provide a complete cross-cultural check for every psychological generalization and therefore the boundaries between cultural anthropology and nomothetic psychology will probably always remain somewhat fuzzy. But we may hope that they will grow somewhat sharper in the course of time.

This is not, however, the only way in which anthropology and psychology overlap. Some personality traits are found, not in all mankind and not only in a single human being, but in all or most of the members of a single society. This kind of regularity is what has traditionally been called "national character"; more recently it has been dubbed "basic personality structure" by Kardiner and "modal personality" by DuBois (1944 chap. 1). No doubt, as Kardiner's term indicates, the personality-traits which we find in a single individual or in the members of a particular society are often thought of as forming a little machine installed at the centre of each human psyche in earliest infancy by the tender ministrations of the parents, just as culture is often thought of as a set of Platonic ideas (ideals, values or standards) floating in the air over the heads of the culture-bearers. But it is dangerous to define an area of study in terms of a type of inferential construct which happens to be in favor at a particular time. In both psychology and anthropology all we can observe is behavior or its material effects; and therefore our definitions had best be in terms of behavior.

Now the regular appearance of certain personality traits in the members of a particular society is clearly culture according to our definition; equally clearly it is most easily studied by psychologists or by anthropologists trained

in psychological techniques and since it deals with personality its study is also a branch of psychology. Modal personality or national character therefore is a part of culture and not, as some have imagined, the source of culture. Motives on the one hand and values on the other are inferential constructs derived from the same set of observed regularities; it is not surprising that they should show a close correspondence for any given society. Nor do we need to introduce any causal mechanism to explain this correspondence.

This argument is a bit difficult to follow without a specific example. Let us take some tribe, the Hopi for example, who rarely quarrel with each other or fight with strangers. We might say that their behavior is peaceful. Insofar as this is a regularity of behavior in individual Hopi it is a trait of their individual personalities and falls within the domain of psychology; but insofar as this personality trait is regularly reproduced in the behavior of most Hopi it is an aspect of their culture. Using the same behavior as evidence, we may think of each Hopi as having a low or repressed motive or instinct for aggression, installed by his parents in his psyche by precept, example or punishment. Or alternatively, we may think of each Hopi as possessing the value "peace." But we do not need to explain the value "peace" as being caused by the repression of the aggressive instinct, since the behavior in each case is identical. Basic personality structure is not the cause of culture or vice versa; it *is* culture or a construct based on culture. All we really have observed is that the Hopis are peaceful and teach their children to be peaceful; in other words that culture or this aspect of it is learned, which is not really a very surprising discovery (cf. similar views in Spiro 1951).

There is, of course, a legitimate use of psychology to explain culture, but we shall return to that later. For the present let us be satisfied to observe that there is a field in which anthropology and psychology overlap, something similar to such borderline sciences as physical chemistry and biochemistry. This particular branch of learning has been called "Culture and Personality" or "Personality *in* Culture" (Kluckhohn and Mowrer 1944: 1), but there is something awkward in using a conjunction or a preposition in the name of a science. Perhaps the best name would be "cultural psychology," as Kroeber has suggested (1948b. chap. 15).

Our definition of culture, then, has helped us to understand the relations of anthropology with sociology, history and psychology; perhaps it will also help us to clarify the question of the causes of culture. But first of all we must try to understand what it is that we mean by a cause.

In common practice and in common speech when we point to some phenomenon as the "cause" of some other phenomenon, what we do is to select some antecedent feature of the total situation which we and our interlocutors know to have also occurred in other similar situations. When a house burns down, we note that there was a thunderstorm shortly before and we surmise

that it must have been struck by lightning, as has happened many times just before a house caught fire. Being struck by lightning was, we say, the cause of the destruction of the house. We do not customarily hunt for all the recurrent features of the situation, but we are quite willing to admit them if some one points them out. We would accept as additional causes the fact that the house had no lightning-rod, that it was built of wood, had not been painted for several years, was empty, isolated, and so on. But usually we mention as causes those antecedent features of a situation which are particularly striking or could have been altered by human agency.

A cause, then, is an antecedent feature of some recurrent process. It is true that we often imagine that there has been some sort of flow of energy from cause to effect; the billiard cue, we feel, pushes the billiard ball. It is probable that here we are merely projecting into our image of the situation the feeling of effort we have when we intervene actively in nature; we think of the muscular tension which we discharge in pushing the billiard cue as flowing through the cue into the ball. But, as Hume showed conclusively long ago, we can never observe any connection or flow between cause and effect; all that we can prove is their recurrence as part of a regularly recurrent sequence of events.

It is perhaps this anthropomorphic element in the notion of cause which has led to its being tacitly abandoned in the more developed sciences. The physicist scarcely can be said to look for causes any more; he seeks to observe regular concomitant variations in measurable aspects of physical phenomena. The biologist, too, looks for regularities rather than causes, regularities of structure in genera and species as well as regularities of process in the life-histories of members of particular species. Just as the notion of regular formal structure (or "synchronic regularity") has replaced the Greek notion of an entity composed of matter and form or substance and qualities, so the notion of regular process (or "diachronic regularity") has replaced the simple notion of causality. The concept of regular process is at once broader and more refined than that of cause-and-effect; it includes all that is useful in the older concept and discards its confusing anthropomorphic implications. We could hardly speak of the egg as being the cause of the chicken—there is a continuity of development here such that no sharp line can be drawn between the two "entities." Nevertheless, the regular development of chicken from egg is a feature of order in the universe and therefore helps us to understand and explain a mature chicken.

When Kroeber (1948b: 411) recommends that the student of culture should content himself with describing sequences of significant forms rather than seek for specific causal explanations, he is in fact proposing that anthropology should follow the procedure of the older sciences. We might add that such recurrent sequences of phenomena are the only form of "diachronic" explanation that science can discover; whatever "causes" in the vulgar sense are em-

pirically demonstrable will be included as antecedent portions of these regularly recurrent processes.

In science, then, the concept of causality has been superseded by the concept of recurrent process or diachronic regularity, and in searching for the causes of a particular cultural phenomenon we should look for recurrent processes which include the phenomenon as one of their terms. Among such processes there will certainly be some which are nothing more than sequences of cultural forms—regular processes of cultural change within a single society such as every anthropologist is familiar with. There will also be processes involving the culture of more than one society—processes which fall under the general headings of diffusion and acculturation.

But is this all? Must we hold, as White does (1947) that only culture can be the cause of culture? That we should only look for regularities of process within the domain of culture itself? Surely this is a needlessly restrictive point of view. The recurrent processes for which we are looking may of course include any features of the universe which are commensurate with those features we call culture. Culture has been defined as regularities of behavior in the members of a society (excluding those clearly hereditary). Among the features of our recurrent processes we may now include those same hereditary regularities which we formerly excluded. We can explain certain aspects of culture in terms of physiological needs common to all mankind, such as hunger, and others in terms of universal psychological motives or patterns. Malinowski, for instance, assumed a universal fear of the unknown to explain the universality of religion, and indeed most functional explanation is of this kind. The prevalence of suspicions of witchcraft among the Zuñi has been explained by the "frustration—aggression—projection" formula, a psychological pattern which we find among ourselves and presume to be universal. Culture, then, can be explained (at least in part) by human nature. We can look for regularities of behavior, physiological or psychological, common to all mankind which fit into regular patterns or processes with the regularities of behavior of the members of a single society. No doubt such explanations can never be completely sufficient; they cannot explain the differences between the cultures of two societies, but they can explain the similarities.

In the common physical or physiological structure of mankind we also have a type of regularity common not only to the members of a particular society but also to the members of all societies. The shape and structure of the human foot serves to limit the possible forms of foot-gear; the location and shape of the genitals helps—along with certain presumed universal psychological motives such as modesty and its correlative, sexual exhibitionism—to determine the widespread use of genital coverings and the common aspects of their shape.

So far we have mentioned only regularities common to all mankind—

bodily structure and human nature. But there are also regularities of structure (and perhaps of behavior) which are thought to be hereditary and which are shared by large segments of mankind including all the members of particular societies. These are the phenomena known as race. Usually the physical differences of races are too trivial to be used as explanations of culture, but we can imagine that the small stature of pygmies permits them to provide lower roofs for their shelters than we do for our houses, or that if the Hottentots were to make chairs, they might invent a special shape to fit steatopygous women. If there are any psychological differences of race—a question which is now neither proved nor disproved—such differences could certainly be used in explaining the culture. The hypothesis that all civilizations were founded by Aryan conquerors is a perfectly legitimate hypothesis, though it happens to have been disproved by the facts. It might conceivably be possible to show that a selected sample of Chinese children, brought up under precisely the same conditions as a sample of European children, nevertheless differed from them psychologically in a uniform way, and such a psychological difference would then help to explain the difference between Chinese civilization and our own.

Race, then, must be added to our list of the possible causes or explanations of culture. We seem now to have exhausted the possibilities of regularities in the structure and behavior of large groups of human beings commensurate with those regularities we call culture. But what of the non-human world? Does it not also have regularities commensurate with culture? We do in fact often conceptualize the world around us in just this way. We think of a certain geographical area as having fauna, flora and physical aspects which affect all its inhabitants in a regular way. This is what we call the environment; it might be defined as regularities in the behavior of the non-human world towards the members of a particular society. It differs, of course, from the environment of an individual which is conceived of in such a way that it presents particular features to him and to him alone. If properly conceptualized, then, environment can certainly be included among the possible explanations of culture.

How does our list stand now? Culture, we have said, must be explained by the empirical discovery of regularly recurrent processes and among the features of such processes we may include not only items of culture but other commensurate regularities, such as bodily structure, human nature, race (including physiological and psychological factors) and environment. All these of course have commonly been used, implicitly or explicitly, by anthropologists, sociologists and historians in explaining particular features of culture. But what of the other mode of explanation so much favored by historians—the influence of a great man? Can a great man or rather his behavior be said to cause culture?

Here the answer must be a firm no. There is an old rule that a cause must be adequate to its effect, and, translated into our own terms, this means that

the parts of a recurrent process must all be commensurate with one another. They must all be abstractions of one kind—all belong to a "homogeneous field of enquiry." The individual or a particular act of his behavior or some idiographic regularity in his behavior—all these are abstractions of different kinds and all differ from the regularities of behavior in a group of persons which constitute culture.

Indeed, the principal difficulty with this problem in the past seems to have come from the fact that culture was conceived of as the sum of the regular behavior of a number of individuals rather than as the *regularity* of their behavior. It is easy enough to think of the behavior of an individual as influencing, causing or forming part of a recurrent process with the behavior of one or more other individuals, but it is much more difficult to conceive of the behavior of a single individual as causing the *regularity* of the behavior of many others. Let us take a specific example, such as Darwin's publication of *The Origin of Species*, which was followed after some time by the acceptance of the doctrine of biological evolution by the majority of educated Westerners. If we examine the facts we see that each individual who read the book or heard about the doctrine reacted in an individual way. Huxley was tremendously enthusiastic and went about England lecturing on the subject; a number of bishops were firmly opposed and preached sermons against this new heresy, and so on. Each man's reaction was determined by his individual history, his character and the individual circumstances under which his opinions had been formed. But the regular aspects of their reaction was determined by the regular aspects of their histories, that is, by culture.

We might consider an analogous problem in the physical sciences. A solution of salt can be brought to a degree of saturation where almost anything—a slight tremor, a tiny change of temperature—will be followed by precipitation. If we try however to go on and imagine a degree of saturation such that the addition of a single molecule of salt will cause precipitation, we find that we have to define the precise position of each ion in the solution before we can obtain a picture of the situation which might conceivably be repeated. In other words, we find that we are still dealing with individuals. It is far more satisfactory to define saturation in a rough and general way, and to say that the saturation itself in combination with other gross aspects of the phenomena causes the precipitation. If we disregard the movement of individual molecules we have some hope of obtaining a formula which will describe the recurrent aspects of the process. Saturation, of course, is thought of to some extent as being the sum of the behavior of a large number of molecules; but if we take concepts such as heat and pressure, which are thought of as regularities in the movement of large numbers of molecules, we cannot even conceive of the movement of a single molecule as affecting them at all.

The behavior of a single individual then is simply incommensurate with

the regularities we call culture. Sometimes in reading the writings of the more inveterate hero-worshipping historians, one gets the impression that they believe that some men—statesmen, artists or thinkers—have a special quality called greatness which somehow makes them larger than life and equivalent in weight and importance to the whole mass of their fellow-men. You are given the impression that the great mass of mankind intuitively recognizes the greatness of a great man and therefore his opinions are adopted, his works admired or his commands obeyed without any consideration of what the specific character of those opinions, works, commands may happen to be. It is as if most people believed in evolution simply because they knew that Darwin was a great man. But surely the greatness of a great man is nothing more than the fact that his opinions have been accepted or that his political leadership has been followed. It is true that such men also often have exceptional gifts, but so also do many men who live and die in obscurity. The greatness of great men is nothing inherent in their characters. We simply project upon our image of them the regularity of other men's favorable reaction to their deeds; their greatness *is*, in fact, culture.

Since individual behavior and culture are incommensurate with each other, it follows that individual behavior cannot be part of a recurrent process involving culture; it cannot cause culture. But the reverse is also true; culture cannot cause individual behavior. We often speak as if it does, but if we examine any particular case, we see that an individual's behavior must be related to his own past behavior or the behavior of other individuals; children learn from their parents, their teachers and their fellows, not from the society as a whole. This fact has sometimes been expressed by saying that "culture is mediated to the individual through other individuals," but even this is a loose manner of speaking in which we call individual behavior which is regular by the same name as its regularity. Of course, sometimes individuals come to conclusions about the culture of the society in which they live, and act on it. We can hear the New England spinster saying: "People don't do that sort of thing here." But even here culture can only be said to be a cause in the same way that the principles of geometry are the cause of the behavior of the surveyor. It is rather the concept in each case which is the cause, a concept formed by observing individual behavior.

There are certain situations in which the actions of a single individual are regularly followed by the uniform or regular behavior of a large number of other individuals. Caesar orders "all the world to be taxed," for instance, or a colonel orders his regiment off the field. But these situations are closely defined culturally, and the individual ruler or commander is fully aware of the cultural definition of his role. The regularity of the reaction of subordinates is therefore fully determined by other cultural regularities, their training or education in the roles which they are expected to play. Here again it is culture that

determines culture; if Caesar were to order his subjects, or the colonel his troops, to stand on their heads, nothing but confusion would follow.

It is not inconceivable that all items of culture might eventually be explained in terms of hereditary regularities—human nature and race—in interaction with differing environments. Cultural anthropology would then, in a sense, be reducible to psychology. But not, it should be noted, to individual psychology, only to those branches of psychology (general or racial) which deal with regularities in the behavior of large numbers of human beings. Whether such a reduction will actually be successful remains to be seen; certainly so far reductionism has been no more than partly successful in any of the other sciences.

There seems, in fact, to be an inevitable tendency, when we are dealing with specific cases, for us to seek to explain items of culture in terms of other commensurate regularities and items of individual behavior in terms of other individual behavior. Bidney, for instance, who is one of the strongest supporters of the view that "culture is caused by individuals," nevertheless seeks to explain the specific case of "cultural crises" entirely in terms of culture or other commensurate regularities (1946). "Change," he says, "whether of society, culture or environment, may induce cultural crisis, and cultural crises may in turn stimulate invention or acculturation." It is curious to note that, although invention is usually brought forward as one of the primary instances of the individual's contribution to culture, Bidney himself treats invention as a cultural phenomenon explicable in terms of other cultural phenomena and thus gives the whole show away.

A little further down the page, Bidney attempts to reintroduce the notion of free-will in a passage which deserves to be quoted in full:

"If on the other hand, one adheres to the humanistic view submitted here, that man, under God, controls his own cultural destiny and is free to choose and realize the ends he would achieve, then he can be certain that much thought and effort will be expended by all who share this view towards the improvement of our human condition. It is precisely because of our belief that man can and ought to decide for himself the form of his future cultural development, that we can predict within limits, and with some measure of certainty, the course of events in the near future." (1946: 541)

This is an exceptionally splendid example of a *nonsequitur*. For if men really are free to choose their ends, there is no particular reason why they should choose the improvement of the condition of mankind any more than any other end. What Bidney is in fact doing is to stress the importance of certain moral standards as an element in our cultural tradition, and to point out that this fact may be used as a basis for partial prediction. And his whole argument is conducted in terms of culture and human nature, as is evidenced most clearly by his use of the generic term "man."

Coulborn too (1952: 115), in his argument on behalf of the individual, surreptitiously, or perhaps rather, unconsciously reintroduces the notion of broader regularities commensurate with culture. "If it had been Wallace and not Darwin," he argues, whose formulation of the doctrine of evolution won general acceptance, "there would have been a small difference, but, if for a series of, say, a dozen Darwins, there had been a dozen Wallaces, or even for a good part of the dozen there had been Wallaces instead of Darwins, then at any point far along in the development of the culture pattern the difference reached could have been great, and in all probability, would have been great." He concludes then that "this reasoning may be extended into a presentation of culture as an endless regression of contributions by human beings backwards in *saecula saeculorum*."

Now we know as a matter of fact that Wallace's theories were presented to the public earlier than Darwin's. If Darwin's theories were more successful in winning general approval, it seems likely that it was because they fitted in better with the general pattern of beliefs and prejudices current in Western society at that time. The difference between Wallace and Darwin then is not irrelevant. But let us accept the possibility that there might have been a dozen Wallaces rather than a dozen Darwins. In all probability then—Coulborn remarks in a footnote that he uses "probability" in the statistical sense—the Wallaces would have differed from the Darwins not in any consistent way, but at random, and these random differences would cancel one another out so that the total effect would be *nil*. But if the Wallaces differed from the Darwins in some consistent way, then this difference would itself be a regularity commensurate with culture—it would fall under the general headings of culture or race or human nature—and we would no longer be talking in terms of individual contributions. Thus, however we may argue, in whatever direction we may turn, we always find ourselves back explaining culture in terms of regularities commensurate with it. We seem here, indeed, to be confronted with something like a necessity of thought.

There is no need for us to deny the existence of free-will. That men make choices, that they sometimes discover novel means of expression, novel solutions to long-standing problems seems to be an immediate datum of experience, and free-will, in this sense, undeniably exists. But no one doubts that men's choices are always partly determined by the unalterable circumstances with which they are confronted and by their own predispositions towards particular ends. The scientific student of man—the anthropologist, the sociologist or the psychologist—seeks to discover precisely how far they are determined. As a scientist, he must deal with the order, the regularities in his chosen field, and the irregularities, the novelty or originality of certain human actions, must always escape his net.

While it is desirable for the scientist as a matter of practice to assume that

any particular phenomenon under investigation is fully determined in order that no feature of order should escape him, it is no longer possible to maintain the general proposition that all phenomena are fully determined. For this would mean, if we translate it into our new terms, that the universe is completely regular, and the mathematical logicians in their recent investigations have clearly shown that such a proposition is meaningless. Any system of order, existent or imaginary, must have some arbitrary elements in it, if only in its premises. A completely orderly universe is inconceivable; it could not exist. We may of course maintain, as some theologians have done, that the phenomenal universe is fully determined by God, that the Diety is the first and universal cause. But in so doing we simply refer the arbitrary or disorderly aspects of the universe back to God. It is for this reason that the problem of evil admits of no logical solution, but only a mystical one.

The universe then cannot be fully determined or completely regular; the champions of free-will need never fear that the scientist will win a final victory. On the other hand, the converse is also true. A completely disorderly universe or realm of experience is equally inconceivable; as the logicians have shown, even chaos must have some structure. The scientist may therefore be sure that there is some order, that there are some regularities to be discovered in any field he may choose to investigate.

As we have seen in the case of Bidney, free-will is often conceived of not as being really free, as an element of disorder or novelty, but rather as a sort of assurance of the primacy and efficacy of certain moral standards. Coulborn too, seems to regret the diminished importance assigned by Kroeber to the individual chiefly because of the salutary influence which reading about the lives of great men has on the young. The point of view of the moralist is certainly a valid and necessary one for the conduct of private life, even though it is a different point of view from that of the scientist. And no moralist would deny that there are limitations on the power of the individual to make his moral standards prevail; he would insist rather that we must go on striving whatever the difficulties. The scientist, on the other hand, must inevitably treat moral standards as simply another aspect of culture, to be explained just as any other aspect is explained. Nor need Coulborn fear that his students will cease to be inspired by the example of great men. After all, it is no little thing for a man to have been the first to see the necessary solution to a grave political problem, or for another to be the most skilled in giving aesthetic expression to certain cultural or universal human tendencies. Such men are justly acclaimed, and will continue to be honored through the ages.

Let us now summarize our argument. We have seen that the cultural anthropologist, if he is to be a scientist, must define culture as some sort of regularity, and we have chosen to define culture as regularities of behavior (excluding those which are clearly hereditary) among the members of a single

society. We have replaced the naive or common-sense notion of causality by the scientific notion of regular recurrent process and we have substituted, for the old rule that the cause must be adequate to the effect, a new rule that recurrent processes must be composed of commensurate abstractions or phenomena. We have then deduced that culture must be explained by culture itself or by other commensurate regularities: bodily structure, human nature, race (including both physiological and psychological features) and environment. We have shown that individual actions cannot be considered as causing culture, and that even the advocates of this position tacitly reintroduce the notion of culture or some regularities commensurate with it. And finally, we have pointed out that nothing in this argument need undermine the belief in free-will or the adherence of the moralist to his moral standards.

Very similar views to those we have put forward have been expressed, explicitly or implicitly, in the past by other writers: Steward (1949: 1–7), Herskovits (1948: 608–621), and Kroeber (1917, 1944). If their views have not won general acceptance, it is perhaps because they have not made sufficiently clear how their arguments are derived from commonly accepted doctrines in epistemology and the philosophy of science. It is to be hoped that in so doing, at the present time, we have been able to make a genuine contribution to the clarification of the problem.

In the last half century American anthropologists have tended, under the influence of Boas to limit themselves to the descriptive and classificatory sides of anthropology and to neglect what is surely one of the most important, perhaps *the* most important problem confronting the student of man: the elucidation of the flux of history. No doubt this self-limitation was justified by the scantiness of our knowledge at the beginning of the century, but the vast accumulation of data, anthropological, archeological and historical, in recent decades seems to justify a new attack on the dark fortress of our ignorance. Toynbee and Kroeber have reawakened our hopes, and if, as Spengler maintained, historical change can only be understood as cultural change, it may be that it is the anthropologists who possess the conceptual weapons and the training necessary for a victorious assault. The preceding discussion is only a modest attempt to clean and sharpen some of these weapons before the attack begins.

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Adoption on Mokil[1]

By J. E. WECKLER

THE adoption of children is a major means by which adults can alter the membership of that basic social unit, the elementary family. We may consequently expect that, where adoption is frequently resorted to, it will have important effects on the structure of the society and on the personality dynamics of the people involved. The comparative study of the role of adoption in diverse societies will increase our insight into the functions of the family in society. Polynesia and Micronesia offer one of the most fertile fields for such investigation, since adoption is known to be particularly common there. Although many anthropologists have mentioned adoption as an important institution in Oceania, there is no report of a detailed study of the phenomenon for any Oceanic society. Raymond Firth, in *We the Tikopia*, gives us our only general discussion of the practice in Polynesia; there is nothing comparable for Micronesia. The present paper is offered as a starting point for a comparative study of Oceanic versions of the institution. Perhaps students of other areas will add their portions. Ultimately we may develop insights that will illuminate the role of adoption in our own society where it is becoming increasingly common and is recognized as a social problem.

Mokil lies in the eastern Caroline Islands, at 6° 39′ N. and 159° 53′ E., about eighty-five miles easterly from Ponape. It is a tiny shield-shaped atoll with maximum dimensions of a little over two miles north and south and about one and a third miles east and west, inclusive of the lagoon. The total land area, less than half a square mile, is divided between three islets: Karlap, Urok and Manton. Karlap is located on the northeast sector of the reef; the Mokilese live along its lagoon shore where they are protected from the prevailing northeasterly trade winds. Most of the dwellings are concentrated on the northern half of the islet but there are scattered houses to the extreme southern tip.

Taro and coconut are the main vegetable foods, but breadfruit, bananas, pandanus and arrowroot furnish supplementary and mainly seasonal variety. Coconut trees grow nearly everywhere, intermingled in the interior with breadfruit. The main taro pit is located on Karlap but numerous smaller ones have been dug, mostly in recent years, in the interiors of the other two islands. The coconut land is divided into nearly 350 separate parcels and taro land is likewise divided into a large number of tiny plots. Both kinds of land are individually owned but all the plots under the control of one extended patrilocal family are worked as a unit under the direction of the family head. Family holdings are characteristically widely scattered and most families own some land on each of the three islets.

In 1947 the population of Mokil was 425. The present inhabitants are all descendants of 25 or 30 people who survived a prehistoric typhoon that occurred around 1775. I secured a complete genealogy tracing the descent of all the present Mokilese from those early survivors plus a few immigrants from other Micronesian islands and casual white men, mostly members of mid-nineteenth century whaling crews.

Nearly a third of the children born on Mokil since the prehistoric typhoon have been adopted. Almost twice as many boys have been adopted as girls. 115 of 178 known adoptees were boys, 63 were girls. This disparity is due partly to the emphasis Mokilese place upon patrilineity and the resulting desire of every man to have sons to "carry on his line," as the Mokilese say. Males are also more in demand than females because their work is considered to be more valuable and to require more skill than the work of women.

Most adoptions are made shortly before the birth of the adoptee or within a few months after that event, but the child remains with its mother until it is weaned, usually during its second or third year. The following table summarizes the data on age at time of adoption for both sexes:

Age at time of adoption	No. of boys	No. of girls	Total
At or near birth	77	44	121
Post-weaning but pre-adolescence	11	6	17
Adolescence	5	2	7
Young adulthood	4	0	4
Totals for whom age is known	97	52	149

These figures show the Mokilese are faithful to a principle of which informants are quite conscious: a child should be adopted early in life so it will grow up regarding its adoptors as true social parents.

Two kinds of adoption are recognized on Mokil. True and honorable adoption is called sheri shoyshoy (pronounced *ʒɛ'ri ʒɔi'ʒɔi*. "Typewriter orthography" seems less distracting than phonetic symbols so I shall use it with apologies to linguists). In this relationship the adoptor is expected to treat the adoptee as he would his own child. The adoptee has the right to expect that he will be loved, treated with consideration, and given land by the adoptor when the former marries or when the latter approaches death. In the second, and much less desirable form of adoption, the adoptee is called a shotay (*ʒo'tai*), translated with a sneer as "working man." The shotay is a dependent with a servant-like status who works for his adoptor for subsistence without hope of inheriting land or receiving rewards beyond the bare necessities of life. An adoptor may mistreat a shotay in various ways without incurring adverse

public opinion. The shotay may be required to do unpleasant tasks at inconvenient hours, he may be inadequately housed, clothed and fed, and his opinions and wishes receive little heed from the community—he is an inconsequential subordinate.

Adopted children know who their real parents are and maintain close and affectionate bonds with them and their paneyney (*pa nei' nei*, the most important social unit, the patrilineal patrilocal extended family).[2] The parents bring food and presents to the child and the child visits them at frequent intervals. Even if the child remains a permanent member of his family of adoption he has lifelong ties of mutual help and support with his parents and their paneyney. Particularly during the child's minority his parents watch over his interests and intercede if they feel he is being misused. They can and will take him back if he is abused or if he and the adoptor prove to be incompatible.

Speaking only economically, children are liabilities until late pre-adolescence and become definite assets during adolescence. If an adoptor rears a child through its unproductive years into adolescence he ordinarily has a lifelong claim on the adoptee's assistance at various tasks even if the latter returns to his paneyney of birth. If a parent takes his child away from an adoptor during the child's early years, however, the adoptor will have little or no claim on the child afterwards. In any case, if the adoption terminates in bitterness the persons involved may sever relations and refuse to acknowledge further mutual obligations.

Parents are motivated to permit some of their children to be adopted and removed from their households mainly by ideals of family structure or by hope of economic gain for the child or for their paneyney. Parents ordinarily feel flattered when a close relative or any couple with whom they are on intimate terms asks to adopt one of their children, and they usually accede. They expect that such an adoption will strengthen still further the bonds between the two families. They can refuse such a request, however, and commonly do if they suspect the prospective adoptor is seeking some excessive personal gain through the proposed relationship. Impoverished parents with many children will sometimes even agree to shotay adoptions in the hope that their children will receive better nurture than they can provide.

Several motives lead Mokil adults to adopt children. Many adoptions are based primarily upon ideals of family structure without concern for and sometimes contrary to the adoptor's personal economic advantage. The desire to help others (usually impoverished relatives), or to reward someone for services rendered are other motives that usually work contrary to the adoptor's economic self-interest. In some sheri shoyshoy adoptions, however, the adoptor may be primarily motivated by hope of personal advantage, and this is often his attitude toward shotay. Several motives of different types are usually

involved in any given adoption. I shall discuss the various kinds of motives and aspects of situations separately, however, in the interest of clarifying the various functions played by the institution.

Foremost among the motives for adoption based on ideals of family structure is the universal desire of Mokil married couples to have children. They clearly adhere to the principle that children are essential to transform a marriage into a family. *Every* childless couple during the past 175 years has adopted one or more children if the marriage endured even a few years. Several young married couples have adopted children during the first few months of their marriages. If the adoptors never have children of their own the adoptee, who is usually a boy, will probably inherit the bulk of their land. He will then be regarded by future generations as having carried on the patrilineal line of the male adoptor (or of the female adoptor's father if the bulk of the family land came from him).

Should the foster parents have children, particularly sons, of their own after they have adopted a boy, two important principles of Mokil kinship structure come into conflict. One is the emphasis upon primogeniture whereby the eldest son ideally assumes headship of the paneyney in the next generation and inherits the largest share of land. The other is the evident fact that, despite the ideals of sheri shoyshoy adoption, the Mokilese feel closer to their own children than they do to adopted children. Thus, in nearly all cases, a man's own son will ultimately become head of the paneyney even if he is born many years after his father adopted a son.

Because every man wants a son to perpetuate his line the first child adopted by a childless couple is usually a boy. This structural motive is sometimes satisfied, in families having no sons, by a man's persuading his eldest daughter's husband to join his paneyney. The Mokilese regard such an arrangement as somewhat akin to adoption. The son-in-law is promised the headship of the paneyney in the next generation with the understanding that his descendants will count themselves as descendants of his father-in-law.

A third motive for adoption that is concerned with the structure of the elementary family is the Mokil view that every man must have a sister and every woman a brother. The chief reason given for this need is that a Mokilese gives his word of honor by swearing on the good name of his (or her) sibling of the opposite sex.[3] It is particularly important that men be able to take this oath for it is they who are mainly involved in dealings with people outside their paneyney. The word of a sisterless man cannot be taken seriously. Hence a father who fails to provide a sister for his son is severely criticized because he has wantonly handicapped his son in future adult affairs. A dozen or more girls have been adopted for this reason during the known history of Mokil. Two unrelated children adopted into the same family are thereafter considered to be siblings.

Another structural, or perhaps purely emotional, motive for adoption has its

roots in the frequency of extra-marital sexual affairs on Mokil. Several married men have adopted children who are widely suspected of being their own through illicit unions and whom they fear to acknowledge. (Wives are troublesome about such matters on Mokil, too, but adultery is not an almost automatic cause for divorce there as it often is in our society.) Conversely, if a man's wife bears a child he knows or suspects is not his, he will readily give it in adoption, even as a shotay.

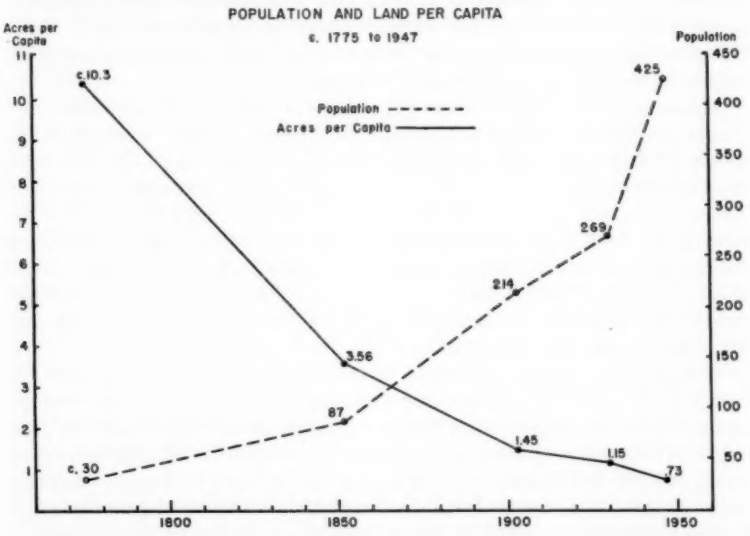
Middle aged people frequently adopt offspring of their married children when they still have immature children of their own. When the adoptee is the child of a son the motivation seems to be primarily sentimental, for no change of residence or household make-up is involved. The child is expected, however, to be particularly close to the grandparents. A more obvious strengthening of social bonds occurs when the adoptee is the child of a daughter and comes back to grow up in the household of his maternal grandparents. People sometimes use this device to pass extra land to a favorite child by adopting an offspring of the favorite and bestowing land on the adoptee. This indirection minimizes protests from other interested parties because of the acknowledged propriety of giving land to an adopted child.

Adoption is also used to establish artificial kinship bonds with unrelated people on or from other islands. Several Mokil families have exchanged children with unrelated friends on Ponape, Pingelap and Kusaie; a few even with Marshallese and Gilbertese families. After such a child exchange the families involved may visit one another for months at a time and thus have a home away from home. In such adoptions the children usually remain with their own parents, paying the adopting parents extended visits from time to time. When occasional Mokil children have been sent to school on Ponape, arrangements have been made for some Ponape family to adopt them, thus insuring the child a home and supervision.

The people of eastern Micronesia have probably always been able to move from island to island as individuals or in small groups. Two of the twenty-five to thirty survivors of the 18th century typhoon on Mokil were the children of a Pingelap couple who had previously moved to Mokil. During historic times, at least, this mobility has been possible only via adoption or marriage, since the right to utilize land and its products can only be acquired through kinship claims. Today many Mokilese trace their ancestry in part to individuals from one or another of all the island groups in eastern Micronesia. These outsiders were all integrated into Mokil society either by adoption or by marriage or both. In later times whenever a person from one of these islands has come to Mokil, the Mokilese who have kinship ties on that island (not necessarily with the particular individual), have felt obligated to sponsor and provide for the newcomer. In many cases the individual or his children have been adopted and permanently absorbed into the Mokil population.

The manipulation of land is often an important motive in Mokil adoptions.

It is almost fair to say of the Mokilese that family land is part of the family structure. They feel particularly attached to land that has passed from father to son through several generations. The intensity of these feelings has doubtless increased considerably in recent generations. There are two main reasons. The first is that during the past seventy-five years the Mokilese have given up their aboriginal communal use of the products of the land and have adopted Western ideas of restricting the utilization of land to its owners. The second reason lies in the exceedingly rapid increase in population which by 1947 had reduced the average holding of land to .73 acres per capita. The accompanying figure shows the relationship between population and acres of land per capita as it has developed over the past 175 years. The poorest family on the island in 1947 was trying to eke out an existence on .24 acres per capita. Under these circumstances concern about the ownership and transfer of land is often a primary consideration in adoptions.



Several Mokil paneyneys own land on other islands, notably Ponape. Some men of the paneyney, with their wives and children, will then live on the other island. They nearly always exchange sons with the men of the paneyney who remain on Mokil. The primary purpose of these adoptions is to have constant reminders of the absent ones at hand and thus to maintain a close family feeling. There are economic overtones, too, because the exchanged children become heirs to their fathers' shares of the paneyney land on each island.

Sometimes a Mokil woman marries a man from another island and goes to live with her husband's people. She may send a son back to be adopted by her brother. Unless the brother has requested it, however, or the woman has a large dowry of Mokil land, her son will be in danger of becoming a shotay.

Adoption has been used many times by conscientious people to return land to the heirs of its traditional patrilineal owners and on other occasions by selfish people to circumvent such an eventuality. If the land held by a childless couple has come mostly from the wife's people the couple is expected to adopt a boy from one of her close patrilineal relatives and settle the land on him. If half or more of their land has come from the husband's people, however, the couple is obligated to adopt at least the first child from the male side of the house and to bequeath the bulk of their holdings to him. Failure to do so results in public censure and creates a situation in which collateral patrilineal heirs may appropriate the land that has been alienated from the patilineal line. Persons both ruthless and clever have sometimes managed to use adoption as a ruse to alienate land from patrilineal heirs the public regards as its rightful owners.

Occasionally a parent, hoping to recover patrilineal land through one of his children, accedes to an adoption against his better judgement or permits it to continue, knowing his child is being mistreated. Sometimes a person adopts a child and gives it land in order to express gratitude to the child's parents for services rendered. This mechanism has also been used at times in culturally disapproved ways, to transfer land over which the donor has only temporary control.

In some sheri shoyshoy adoptions the adoptors are motivated primarily by self interest. These are instances in which the adoptors are seeking current or future "social security." Numerous aging people, usually women, have adopted children, often granddaughters already living in the same household as themselves. The adoptor gives special attention to this child and seeks to build up strong bonds of mutual affection with it. Then when the adoptor becomes enfeebled with age she can expect the adoptee, by now an adolescent or young adult, to return the loving attention by preparing special foods for the adoptor, washing her clothes and person, devoting time to her amusement and generally ameliorating the infirmities of age. Old women frequently need such assurance of comfort and companionship, especially if they are widowed. Daughters-in-law evidently often resent the authority the mother-in-law exercises over them when she is in charge of the feminine activities of the paneyney. As the old woman becomes infirm the younger ones are liable to ignore or even mistreat her. In such situations an adopted child is a great comfort to the aged woman.

Variations on this theme have occurred several times when aging women have had particular reason to feel insecure in their husbands' paneyneys. A

woman who has come to her Mokil husband from another island, for instance, may be very insecure if she becomes a childless widow. The same may be true of a woman who is the widow of a junior member of a fraternal paneyney, even if she has children by her dead husband. Such women can strengthen their positions by creating new kinship ties via adoption.

Men ordinarily need not employ this strategem because, as permanent members of their paneyneys of birth, they enjoy greater economic security than women at all stages of life. The occasional male who does lack secure membership in a paneyney (e.g., an illegitimate son whose father has not acknowledged him) is likely to employ adoption in his effort to attain it.

Many adoptions on Mokil have been based on the wish to help unfortunate relatives. Thus a man with considerable land but few children wins public approval if he adopts children from close relatives (especially patrilineal ones) who are short of land. During the heyday of whaling in the mid-nineteenth century a landless woman named Jalis entered into a succession of temporary marriages with a series of outsiders, some Micronesians and some members of whaling crews. The series of children by these various fathers were all adopted by Jalis' brothers and sisters and given land. Some of Mokil's most solid citizens today, including the man with the largest single holding of land, are descendants of these off-spring of Jalis. Were a woman to produce a series of children by transient fathers today, however, they would probably become shotay, for hardly anyone has enough land to give a waif independence.

Nowadays junior branches of several families are nearly destitute. Close relatives sometimes adopt their children just to help feed them. Such adoptees are shotay in the sense that they are uninfluential dependents and cannot hope for sufficient land to achieve independence. But the adoptors commonly feel strong affection for them and treat them well. Waifs are always adopted by some relative out of pity and a sense of kinship responsibility. Such adoptees usually become shotay.

An unmarried woman with a child is handicapped in getting a husband. This is true whether the child is illegitimate or is the offspring of a prior marriage. A man may propose marriage to her but specify he doesn't want her child. In such circumstances the woman's father or a brother will usually adopt the child who will have the status of a shotay in his household. To reduce the number of unwanted waifs due to illegitimacy the Mokilese have recently ruled that a man must take and rear his illegitimate child. (Paternity is established in doubtful cases by deciding which of the possible fathers the newborn child most resembles.) The society expects that a man will have enough affection for his own child to give it land whereas, since men control the bulk of the land, a fatherless child has little hope of obtaining any.

Occasionally an adoptor, saddled with a child he has taken out of a sense of kinship responsibility, will become fond enough of the adoptee to "legiti-

mize" the relationship by giving land to the adoptee, thus creating a situation the community will accept as a sheri shoyshoy adoption. Commonly, however, the shotay is an inconsequential, essentially unwanted, subordinate. His lot, as the song goes, is not a happy one. He labors all his life merely for his keep and has no basis for hoping, if a male, to become head of his own paneyney, or if a female, to make a good marriage.

Mokil informants imply, and evidence supports them, that the openly exploitive shotay relationship came into existence shortly before 1880. All the aboriginal and early post-contact shotay described to me were adults who came to Mokil from elsewhere and were accepted into the household of the king or of some other important man. All such individuals who served their sponsors well seem to have received handsome portions of land, sufficient to enable them to become respected members of the community. The earliest non-Mokilese now remembered to have been incorporated into the community were the two Pingelap children who survived the typhoon that occurred about fifty years before the European discovery of Mokil. They were adopted by Mokilese survivors and received ample gifts of land. The term shotay is not applied to them. These and all aboriginal adoptions of Mokilese by Mokilese seem to have been of the sheri shoyshoy sort.

A century and a quarter of contact with Western culture has produced many changes in Mokilese social structure, including modifications in the role played by adoption. Aboriginally, when the Mokilese lived on a purely subsistence basis and shared the products of the land in a semi-communal way, adoption had little or no economic import. When they became dependent on trade for essentials like tools and cloth, however, and especially after they accepted, in the interest of trade, the idea of restricting the use of land exclusively to its owners, then the land transfers traditionally involved in adoptions became an important aspect of the practice. The acceptance of trade and of Western concepts of land use also created the need for extra-familial labor among the few who emerged with large holdings of land. Instead of taking over the Western custom of wage work to meet this need, however, the Mokilese developed the exploitive form of adoption. In the next few paragraphs I shall present the salient facts of the economic acculturation and show how they affected adoption.

Whalers, the first important instruments of acculturation, began visiting Mokil in the 1830's and were most numerous in the 1850's. They introduced trade to the islanders, taking native produce in exchange for such items as tools, cloth, sugar, tobacco and liquor. The Mokilese quickly became dependent upon these goods, especially tools and cloth, and were avid traders. Until around 1880, however, they clung to aboriginal economic patterns in their dealings with the white men. Aboriginally, all resources were public property and first fruits had to be brought to the king to be "blessed" before anyone

could partake of the year's crop or the season's first catch of a migratory species of fish. All early trade was likewise channelled through and controlled by the king. People gathered coconuts, bananas and other vegetable foods wherever they chose, caught the half-wild pigs and chickens, and took them to the king to trade. After making the trade the king distributed the proceeds according to need. If several men in one family needed pants they might all get them even if they had not gathered produce on that occasion. If the men of another family were all adequately clothed they would get no pants but might get some steel tools if they could convince the king of their need.

According to modern informants all the land of Mokil was privately owned even before the first contacts with whalers. My accounts of land transfers go back to the original division between the heads of the three families that survived the 18th century typhon. I think the authenticity of some of the alleged early transfers is questionable, however, and it may be that not even pseudo-private ownership came into being until after the first white men began living on the island in the 1830's. At any rate, modern informants old enough to remember events in the '80's are quite definite that until sometime between 1875 and 1880 anyone was free to gather the produce of the land wherever he chose, regardless of property lines. The only restrictions concerned specific trees of newly introduced varieties and planted crops of taro on which an individual had expended considerable care and labor. He had exclusive rights to such produce even if it grew on another person's land, but would ordinarily share it upon request.

A Mokil king voluntarily "converted" his people to Christianity in 1862 and established a native church on the island for which there was no trained minister. American members of the Boston Missionary Society visited the island occasionally and briefly during the next twenty-odd years but did not station an American worker there until 1890. They could hardly have had much influence in favor of private utilization of resources and individual enterprise prior to 1880.

A crucial event in changing the orientation of Mokil culture in its economic aspects was the fact that around 1870 a Mokil man named Zacharaias joined a whaler's crew and was away from the island about ten years. He became fluent in English during that time, visited many of the great ports of the world, and undoubtedly had assimilated numerous new cultural ideas by the time he returned to Mokil. Other Mokilese served as whalers, but none for so long a time, and none of the others became king as Zacharaias did shortly after his return.

The second crucial acculturative event, which occurred within a year or two of 1875, while Zacharaias was away on his long stint of whaling, was the introduction of organized copra trading to Mokil. The earlier trade with whalers, while it had become important to the Mokilese, had always been casual.

The whalers wanted fresh fruit, fresh meat and women for their crews. They accepted pigs, chickens, breadfruit, bananas, green coconuts for drinking, but only a small amount of ripe coconut, mostly in the form of oil. They could not take large quantities of any produce because of the danger of spoilage. Since the population of Mokil during the whaling period never exceeded a quarter of the present figure, much of the land must have been idle; the resources of the island were never strained to supply the whaler's needs. Essentially, the Mokilese were trading an easily produced surplus for the cherished manufactured goods. Moreover, trade with whalers decreased markedly after about 1860 because of the near extinction of whales in the tropical Pacific.

When the New Zealand, American and German copra traders extended their operations westward from Polynesia into Micronesia in the 1870's a whole new vista of economic opportunity opened to the Mokilese. For the first time there was an unlimited demand for coconuts, the major product of their soil. There was reason now to keep the land in full production, to gather the fallen nuts regularly and systematically and prepare them for the traders rather than allowing the uneaten nuts to rot, sprout or feed the coconut crab. Yet, for the first four or five years, the old pattern of trade persisted; anyone who felt like it gathered nuts wherever he chose and brought the prepared meat to the king to trade. At this time only one trader was stopping at Mokil for copra.

In the late 1870's these two acculturative developments came to fruition almost simultaneously. Three or four traders began to compete for Mokil's copra and Zacharaias came home from the sea. He found his younger brother about to inherit the kingship and, asserting his right of primogeniture, took the post for himself. One of his first acts as king was to issue an edict modifying Mokil land tenure in the direction of customs he had observed among the white men. He ruled that henceforth people could gather produce only from their own coconut land, that taking even so much as one banana from another's land would be punishable as theft. He is said to have proclaimed: "If there is anyone who has not enough land let him work for others or starve."[4] At about this same time each competing copra trader appointed a man on Mokil to represent his interests. One firm stationed a white man there for a couple of years but the rest all set up Mokil men as traders from the start. Each paneyney thereafter made copra from its own land and traded it to the Mokil representative with whom it had the closest bonds of kinship.

Under the new conditions a few men found themselves with more land than they could work efficiently. Two of the largest holdings were those of Zacharaias and Charlie Dennis, son of a Mokil woman by a white whaling captain. These two men solved their labor problems in the same way, whether by diffusion or by independent invention I can't say. They each "adopted" seven adolescent or young adult males from poor families and put them to

work making copra. They evidently fed, housed and clothed their working men well. I do not know what verbal agreements the two men made with the families of the "adoptees" but the relationships might easily have appeared to the public for many years to be bona-fide sheri shoyshoy adoptions. (This is implied by modern informants.) Neither do I known whether the two acculturated men used the traditional practice of adoption cynically from the first to get cheap labor or whether it only dawned on them slowly that they could do so at the slight expense of some bitterness among inconsequential people, and muted public criticism. (The Mokilese today, and probably far back in their past, tend more to rally around a powerful man and seek his favor than to smite him in support of the weak.)

It seems clear that Zacharaias placed his seven working men in a special category. A childless man, he had adopted several children before his whaling career. At his death his land was divided among these genuine adoptees and none went to any of those he later "adopted." None of Charlie Dennis' working men received any inheritance, either. (Because I believe these fourteen shotay were not genuine adoptees, I have not included them in the table showing age at time of adoption that appears early in this article.) People still speak of Zacharaias and Charlie Dennis with a mixture of awe and resentment as "smart." I believe these two men invented the modern shotay form of adoption by combining elements of aboriginal adoption and of the aboriginal "working man" relationship, adding to this mixture the exploitive element *foreign to both* of giving the shotay no reward beyond his keep. The derision that attaches to the term and to the relationship nowadays is the complement of the Mokil attitude toward "big men."

Today the Mokilese judge every adoption pragmatically. When the adoptor's behavior toward the adoptee is adjudged by the community to exhibit the raw self-interest of exploitation the adoption is regarded as a shotay or working man relationship. An essential feature of the honorable sheri shoyshoy adoption is that the adoptor shall ask for the child freely and without the existence of any coercive situation. It is nearly as important that he make the request before or shortly after the child is born so that culturally acceptable parent-child relations can be established between him and the adoptee. The adoption of a person past infancy always creates some doubt about the legitimacy of the relationship unless the adoptor immediately gives land to the adoptee or the adoption is the result of some special set of circumstances accepted by the public as a reasonable basis for a sheri shoyshoy adoption. The same doubt exists if a comparatively well-to-do person adopts a child from a poor person who is neither a particularly close relative nor has performed any signal service for the adoptor.

No parent who is able to maintain his reputation of being able to provide

for his family will permit his child to enter a shotay adoption. Nevertheless, the avowedly shotay adoption has some integrative functions on Mokil today. It permits a comparatively poor man to fulfill culturally determined kinship obligations to adopt a waif or the child of still poorer relatives without at the same time committing him to further subdivide his already inadequate land holdings. Families that are too poor to afford pride can permit a child or two to be adopted as shotay and salve their feelings with the hope that the child will be better fed than they could manage.

The vast majority of adoptions are still entered into as sheri shoyshoy relationships. Ancient cultural ideals and expectations still motivate people to participate in adoptions. But the economic facts of life on Mokil today often counteract traditional values. There is more jockeying for advantage by participants, and more concern about the economic aspects of adoption, than there used to be. Parents concern themselves about the economic status and reputation for generosity of anyone who proposes to adopt their child. Poor parents must guard against allowing their child to enter an adoption in which he will be called a shotay. All parents must watch developments in the adopting family and intervene to protect their child's interests if they feel he is being pushed to one side or mistreated. The adoptor's own children frequently resent the adoptee when they grow old enough to be concerned about their own economic futures. The adoptor can always break his word and unilaterally create a shotay situation by failing to give land to the adoptee. His children and other relatives can find ways to poison his mind against the adoptee to that end.

Under these pressures many present day sheri shoyshoy adoptions degenerate into shotay-like relationships that fail to fulfill the expectations of participants. Nearly every adoptee sooner or later returns to his paneyney of birth, often in an atmosphere of bitterness and recrimination. Parents sometimes take their offspring away from an adoptor while the adoptee is still a child. They are impelled to this step by pity if the child is mistreated, but also by pride if they suspect, or people begin to say, the adoptee is nothing but a shotay. Other adoptees return to their paneyneys of birth at or shortly after their marriages because the adoptors fail to give them land on that occasion. Some linger on in the adoptor's paneyney after marriage even under such circumstances, hoping for a deathbed bequest. If they are again disappointed they return to their paneyneys of birth, sometimes as adults with several children of their own, unless circumstances force them finally to acknowledge a dependent shotay status. Some adoptions, of course, still do work out satisfactorily for all participants. But an institution that formerly functioned entirely to increase social solidarity, today often produces discord and dissension.

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NOTES

[1] The field work for this paper was done in 1947 as part of the Coordinated Investigation of Micronesian Anthropology, organized by the Pacific Science Board of the National Research Council and supported by the Office of Naval Research and the Wenner-Gren Foundation for Anthropological Research, Inc. I thank these agencies and the people of Mokil who together made the research possible. I also want to express my appreciation of the comradeship and collaboration of my colleague in the field, Mr. Conrad Bentzen. The present paper is based entirely on my own work but some future papers will utilize data gathered by him.

[2] Aboriginally, matrilineal clans were important social units on Mokil. Today they do not function, however, and are nearly forgotten. Less than half the adults remember their clan affiliations off hand and clan exogamy is no longer enforced. The shift to almost exclusively patrilineal emphasis apparently occurred during the stringent cultural readjustment that followed the prehistoric typhoon and seems not to have been a result of contact with Western culture.

[3] This custom is connected with brother-sister restraint oriented around sexuality, which on Mokil leads to respectful formality between such siblings. Neither may use obscene language nor refer to sexual matters in the presence of the other and it is exceedingly bad manners for a third person to do so in their joint presence. Brothers and sisters do not sleep under the same roof after reaching puberty. Similar rules apply between parallel cousins of opposite sex. A person who lacks a suitable sibling may swear by the good name of such a cousin but this oath is not as convincing as the proper one.

[4] This ruling applied only to coconut land, the source of money income as well as of all dry land crops. Taro land continued to be used in a modified aboriginal way up to 1911. Until the early 20th century the swampland available for taro production exceeded the food needs of the population and some of it was always lying fallow. People who had excess taro land loaned it freely to those who needed more. Theoretically the owner or his heirs could recover the land whenever they wanted it but the borrower was entitled to harvest his current crop first. Actually, as the pressure on the land increased because of population growth, borrowers became more and more reluctant to return land to its owners, especially plots they had been using a long time. Hard feelings and bickering grew to such proportions that in 1911 King Joel ruled that the lending of taro land must cease.

Two Surviving Luiseño Indian Ceremonies

By RAYMOND C. WHITE

MANY factors have contributed to the decline of Luiseño Indian culture in southern California since it came under the influence of Christianity with the founding of the Mission of San Luis Rey in 1798. The observance of native religious rites has not escaped this decline. For instance, the puberty ceremony is reported as having been last performed perhaps ninety years ago (DuBois 1908: 77), and current investigations reveal that little understanding of its proper forms, significance, or secret ritual remains among these Indians.[1]

But one hundred and fifty years of acculturation have not sufficed to snuff out all of the old social structure and religion, even though most of the remaining rites are infrequently observed. Two ceremonies that have persisted are the installation of the religious chief, called *scheiyish noti*,[2] and the clothes-burning ceremony, or *tchoiyish*,[3] having to do with the disposal of the spirits of the dead. These two, attended by a group from the University of California at Los Angeles on the night of Saturday, June 21, 1952, provide a modern version of these relatively infrequent ceremonies.

Even though all of the Luiseños are nominally Christian, the old native religion along with its cosmogony and origin myths is crucial to all that remains of the culture core, and the performance of the clothes-burning ceremony serves as a determinant in the maintenance of social structure as well as in the preservation of Indian values and attitudes. Kroeber (1925: 675) has commented on the intensity with which death preoccupies the Luiseño: "He wails for days for his kin . . . shudders at their mention, but lavishes his wealth in their memory."

This attitude toward death has to do with the Luiseño story of the mythical ancestor, Wiyot, who was the "father of" all things. He provoked the anger of one of his "children," Frog, who murdered him. In the period subsequent to the death and cremation of Wiyot, immortality for his "children" became impossible; for with all of their discussion and effort they, unlike Wiyot, who knew all things, were unable to solve the problem of overpopulation. The Eagle dance, now no longer observed, was in part commemorative of this fact, for Eagle alone escaped death by "giving up his children." (It is said that Eagle must either himself destroy his young, or evict them from the nest so that they perish from some other cause.) Other than adult Eagle, all Wiyot people must die in some manner or another.

Thus, the spirits of Luiseño dead are disconsolate over the loss of immortality and reluctant to leave the region of their lifetime attachment. The clothes-burning ceremony serves to "make the spirits happy" and less reluctant to depart by restoring to them belongings made inaccessible by death. Although it is said that formerly all possessions belonging to the dead, includ-

ing houses and domestic animals, were rendered into spirit form by burning, the current practice is to burn only token possessions, such as a hat or shirt.

Of rather different nature is the associated ceremony in which the religious chief is installed. The observance of the clothes-burning ceremony requires the activities of a religious chief, or *not*^{h}, for providing the specialized and esoteric ritual techniques to get the spirits of the dead Liuseños to depart. Hence, the more secular requirement of maintaining unbroken lines of such chiefs along with their formal organization called *scholash*, or *not*^{h}-road, somesometimes called "chief's visit," is essential. The installation of a new religious chief is central to the public formalization of this institution, and seems worth reporting since it apparently has been described only by Boscana c. 1814 (Hanna 1933: 17–19), and then primarily in principle only.

For clarity, a brief description of the functioning of the *not*^{h}-road is necessary. Each religious chief is the leader of a group of families and individuals termed a "party" (cf. Gifford 1918: 206), and it is for each dead member of the party that the clothes-burning ceremony is performed. But no chief may perform the essential features of a ceremony for his own people. Instead the *not*^{h}-road comes into play. Religious chiefs are permanently paired in this institution of reciprocal obligations, and it is always the other member of the pair who performs at the clothes-burning ceremonies of a particular party. In such activities the religious chief is said to be assisted by the entire membership of his party. Since all of these people must be paid in quantities of food, yards of cloth, and money, the expense involved is too great to permit the observance of the clothes-burning and other ceremonies with the frequency maintained in pre-Spanish days. Although the performing religious chief personally receives a relatively fixed sum for each of the spirits' clothes burned (currently about twenty dollars), the quantities of cloth and food given in payment to the rest of his party does not rise proportionately when there is a mass clothes-burning, hence considerable savings are effected. Furthermore, since no great proportion of the Indian population subscribes to the religious aspects of the party system any longer, the number of spirits thus accumulated is not excessively large, only six being involved in the present instance.

With the decline in Luiseño population and Indian adherence to the old religion, the ceremonial chieftainships have fallen vacant on some reservations, and only four distinct parties with chiefs are to be found upon the reservations of Pauma and Rincon. These four chiefs are linked in two *not*^{h}-roads: The chief of the Anoyum party at Pauma is paired with the leader of the Ivium party at Rincon, and similarly the Soktchum leader[4] at Pauma shares a *not*^{h}-road with the chief of the Majel at Rincon. Diagrammed, the relationship becomes more clear.

Pauma Reservation	*Rincon Reservation*
Anoyum Party	Ivium Party
Soktchum Party	Majel Party

Since it was for the spirits of the Ivium dead that the Rincon clothes-burning of June 21, 1952, was observed, the obligation of performing devolved upon the Anoyum. Furthermore, it was the Ivium who were expected to pay the Anoyum for their services. However, owing to the death of the old Ivium leader less than a year before, it was necessary that the *not*^{h}-road be re-established through the formal installation of a new chief of the Ivium.

Although the *not*^{h}-road or "chief's visit" thus appears to be largely a personalized relationship between each pair of religious chiefs, much more is involved. Chieftainship is patrilineal by inheritance, but the candidate must prove to have a character fitting him for the office. He must be acceptable not only to the predecessor who trains him, but also to the chief with whom he is to practice the *not*^{h}-road. He is formally "accepted" by the latter, not only *de facto* prior to installation, but *de jure* by virtue of it. Furthermore, he must be acceptable to the entire membership of both parties—upon the part of his own because of the nature of his secular leadership, and upon the part of the opposite group because of the necessity for correctness of ritual.

Although the reciprocal relations between any particular pair of religious chiefs is clearly institutionalized, an unusual but not improper practice marked the particular ceremonies in question. Because the Anoyum leader was too old and infirm to fulfill all of his obligations personally, he hired as personal surrogate the religious chief of the Soktchum from Pauma to perform the bulk of the activities, reserving to himself only the general supervision of the rites, and personal performance upon two occasions.

Activities initiating the double ceremony began shortly after darkness had fallen and a fire had been lighted in the center of the bench-lined Ivium *wamkish*, a circular ceremonial enclosure (see Fig. 1). No specialized clothing was noted. For instance, the *not*^{h}-elect was dressed in levi trousers and a white shirt, and his hat was a Stetson with the brim turned up on either side, rancher style.

The installation of the chief was composed of two parts, the first of which was primarily introductory, serving to provide a thirty-minute prelude in which visitors could get located and reasonably quiet, as well as to set the scene and provide appropriate emotional patterns of interest and attention. Members of the Ivium party took their places on benches behind and to the right of their chief-elect, and the Anoyum sat to their left. Seated on the main performer's bench on the south side of the fire, the *not*^{h}-elect and four assistants sang a sequence of short songs, which increased throughout in complexity, range of notes, and tempo. Dances were briefly performed twice, first by one woman alone, and later by five women as a concluding feature of the introductory period. The dancers and singers then retired from the ceremonial enclosure to a small army side-wall tent nearby.

It should be mentioned that of three different types of dances observed the only one performed up to this point was also the most common, and was

usually executed by women. This dance, always appearing to be the same, except for minor features, was a gentle bobbing of the body upon the toes, with the knees slightly flexed, sometimes becoming a series of short hops in position. The body was held stiffly erect with the arms and hands extended as if warming them at a fire. In time with the singing and the beat of a turtle rattle, the torso was rotated upon the hips several times so that the palms faced first south, then west toward the fire, and then north.

By way of beginning the actual installation process, five women laden with rolls of brightly colored yard goods appeared and took stations along the east side of the ceremonial enclosure. Then the chief-elect, ushered by the surrogate

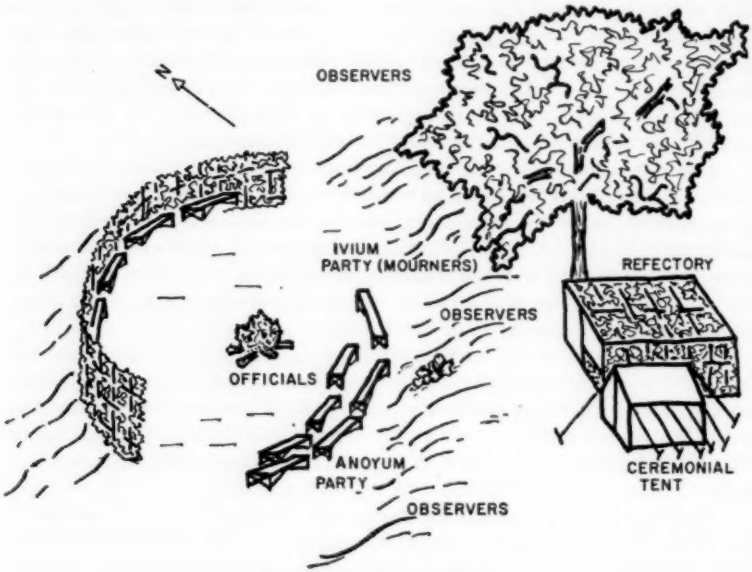

FIG. 1. The Ivium Wamkish.

to the Anoyum chief, entered and began the first of three slow clockwise circuits of the fire. Each time as they passed the women, the latter draped some of their cloth upon the shoulders of the candidate. This act was said to be "something like crowning a king." In the meantime, the new leader was receiving instructions from his conductor, the chief of the Soktchum. These were first given verbally, a form termed *waʰhahish*, and then the same words were rendered in song and called *waʰha*: "Give the older people tobacco if there is no water or bread. Take care of the old people. Take care of your chil-

dren, uncles, aunts, and cousins; take care of your people. Do not be greedy; do not be a hog like Sotello. Poke the fire so that it will burn bright and warm. Keep comfortable."

On the final circuit of the fire, not only the five women, but others of the Ivium party fell in behind their leader, single file. This signified that they would follow him, and would not "throw away" their religion.

Approaching the main performer's bench, the substituting chief removed the cloths, which not only symbolized election and acceptance of their new chief by the Ivium party, but which also became payment to the Anoyum people for services rendered. Then he placed a red bandana knotted at the four corners upon the head of the Ivium chief-elect, and the two kneeled facing the fire. Genuflecting with the arms first down, then upraised, in three series of three, they uttered the growling and grunting sounds, "whah . . .uh." Three is a mystic number for the Luiseño, and nearly all elements of the ceremonies were executed in conformance with this fact. These sounds, so difficult to describe, and termed "growling," "grunting," and, elsewhere, "blowing," are felt to be part of an irreducible minimum without which none of the ritual activities is valid.

As the genuflecting was completed, the five women who had earlier draped cloth around the shoulders of their new leader moved into the vicinity of the fire and threw a shower of coins into the benches of the Anoyum party. During the ensuing confusion, in which children darted about retrieving coins, the two chiefs faced the audience. The surrogate for the chief of the Anoyum, speaking Luiseño, concluded the ceremony with the announcement: "A son, a brother, a cousin, a relation are entitled to inherit. This is what we are doing tonight."

One rather serious omission marked the installation. The *not*^{h}-elect should have been formally introduced to the old and infirm leader of the Anoyum with whom he was to share the *not*^{h}-road. This was the duty of one of the Ivium acting as an assistant to the new leader. The Anoyum chief should have acknowledged the introduction and formally accepted the candidate in public acknowledgment that the *not*^{h}-road was "made again," or re-established. That the Anoyum chief nevertheless accepted the candidate is shown by the fact that no offense was taken at the omission. The way was thus opened for the clothes-burning to take place. Had offense been taken, very serious consequences, other than the breakdown of chiefly organization and the immediate close of ceremonial activities, might have resulted. These would have taken the form of supernatural punishment.

As for the second ceremony of this series, the *tchoiyish* or clothes-burning, it was both more lengthy and more elaborate than the installation of the chief. This central ritual has been described to some extent by Boscana (Hanna: 73) and others (Kroeber 1908: 181, and 1925: 675), but the modern version contained several features not only variant but worth noting. For

example, of the four assistants to the surrogate performing chief from the Soktchum party, three came from the Anoyum and Soktchum parties, as should have been the case, but one member of the group belonged to the mourning Ivium party. Technically this was a serious breach of pattern, for a member of the mourning group should under no circumstances take an active part in such duties as are reserved for the exercise of the *not*^{h}-road relationship in the performing of the clothes-burning ceremony. In this instance, the Ivium assistant was said to have been permitted to help out because his wife was an important member of the Anoyum.

As for the pre-Spanish celebration of the clothes-burning ceremony, it seems then to have been a recapitulation of the death and cremation of Wiyot, the mythical ancestor of all things, in accord with the myth and knowledge associated with this event. Since much of the latter has survived, the rationale for the clothes-burning is still known even though Luiseño ritual in general has suffered severe loss.

The clothes-burning ceremony may be divided into four major sequences: the preliminaries, the "clothes-washing," the calling of the names of the dead, and the "sending" of the spirits. Because of the infirmities of age, only in the first two of these sequences did the old religious chief of the Anoyum participate directly. He opened the ceremony by emitting a series of three long, low, growling sounds. These were answered twice in kind from the preparatory tent, and were followed by the appearance of the hired surrogate and the four assistants. They carried six bright rolls of cloth, each containing a piece of clothing belonging to an Ivium spirit of the dead, and labelled with the name of that spirit. One also carried a small bowl of water.

After three clockwise circuits of the fire, the performers placed the bundles at the feet of the aged Anoyum *not*^{h}, and a large number of mourning Ivium women heavily laden with boxes and bundles of goods who had joined the procession upon the third circuit, took stations along the eastern side of the ceremonial enclosure. The Anoyum chief then rose laboriously and, followed by his surrogate and assistants, circled the fire three times in single file, all the while speaking loudly in Luiseño. He gave consolation and instructions to the dead concerning conduct and the dangers to be avoided in the afterworld.

Then, retrieving the bundles, the surrogate *not*^{h} and the assistants kneeled facing the fire, chanting in unison "a-hei!," "a-hai!," "a-hoo!," first extended the bundles down, then at shoulder height, and finally upward, respectively. In accord with the mystic number, this was done in three sequences of three.

The clothes "washing" began with a song by the surrogate religious chief, during which two women placed bandanas knotted at the corners upon the heads of the six principal officials—a blue one for the Anoyum chief and red ones for the assistants and surrogate. The song was a very important one, which, although no longer translatable and deliberately garbled in rendition,

nevertheless is understood to recapitulate events during the mortal illness of Wiyot. Upon its completion the singer took a spatulate stick, dipped it into the small bowl of water brought into the ceremonial enclosure earlier by one of the Anoyum assistants for the purpose, and brushed water in turn upon each clothing bundle. The assistant then drank the water.

It should be noted that this cremonial "washing" is not to be confused with a separate ceremony performed immediately after the death of an individual and termed *tuvish*. The latter is said to be identical in form to the clothes-burning rite except that no wealth is distributed and the incineration of clothing is absent. A feast is given the "visiting chief" and his party (a function of the *not*^{h}-road), and women dance in the manner described above. This ceremony is said to accomplish two things. The spirit is banished from the immediate vicinity, and supernatural approval is gained in the event that delay due to financial stress may prevent the observance of the clothes-burning one year after any particular demise. The present clothes "washing" seems to be a doubling or reiteration of essential elements in the *tuvish*, and has to do with a pre-Spanish ritual in which a portion of ash and ground bone from the cremation of the particular body was mingled with water and consumed (cf. Strong 1929: 299).

The next portion of the clothes-burning ritual—the calling of the names of the dead—was especially significant in that here the mourning Ivium made the bulk of their payments for services rendered by the Anoyum party. Taking a flashlight, the surrogate chief examined each bundle, unwrapped it, extracted the money rolled up with the clothes (payment for the services of the *not*^{h}), and called the name of the dead. As each name was called the women who had been stationed along the east side of the ceremonial enclosure moved forward and either draped yards of cloth upon the heads and shoulders of the five officials, or deposited boxes of canned goods at their feet. This wealth was passed into the keeping of the proper ceremonial chief for a later equal division and distribution to the membership of his party. With the calling of each name, a short eulogy was given: "Thomas Guassuc! He was an old man. He was among us; he belonged to the Ivium. He is gone now, and we are giving him back his things. We are giving him what he owns. Now he can be happy."

At this point an exception to ordinary procedure appeared—a feature that raised comment. The sixth and last clothing bundle named belonged to the deceased predecessor of the new Ivium chief, but it could not be burned because he had not been dead a full year. Hence this clothing bundle had been merely "washed," and was now returned to the Ivium without further ceremony or payment. Since it is customary to schedule the ceremony so that the clothes of all accumulated spirits of the dead belonging to the particular party are burned in a single observance, the procedure used was not considered to be in accord with prescribed usage.

The "sending" of the spirits of the dead brought the clothes-burning ceremony to a climax. The substituting chief and the four assistants performed first a lengthy genuflection, and then a side-wise shuffling dance clockwise nine times around the fire, all the while continuing rhythmically various thrustings with the clothes bundles. All this was done while the surrogate sang. His song, crucial to the success of the ceremony, started by relating how the body of Wiyot was rolled into the fire for cremation, and ended with a verse describing the fact that everything had been reduced to ashes. Of the nine stanzas, six were connected with the period of genuflection and the remaining three with the dance. After each chanted stanza, twenty-seven thrusts were executed in multiple series of three. Starting chest high, the clothing bundles were thrust three times toward the ground with a deep growling sound. For the horizontal and upward thrusts the sounds were a prolonged "hah!" and a blowing respectively. In withdrawal a vigorous "wah!" was uttered in all instances.

This dance differed markedly from the more common variety described previously, in that it was executed in time with a special song, featured thrusting motions and syllables, and was characterized by a sidewise shuffling motion of the feet while the dancers faced toward the fire. Eight of the nine circuits of the fire were done by the officials, but six women from the wealth distributing group along the eastern side of the ceremonial enclosure joined for the ninth circuit.

With the dance concluded and the last growling syllable uttered, the surrogate *not*^{h} gathered the bundles from the assistants and made a chanting announcement: "They have been here, they have suffered, and now they are going." He then threw the clothing bundles into the fire.

After this the last major act of wealth distribution took place. The Ivium women showered money and cloth not only into the benches of the Anoyum, but also the audience at large. Thereafter, at intervals, they thrust money into the pockets of an assistant, who with a long pole insured complete combustion of the clothes bundles.

While the clothes were burning the mourning Ivium sobbed loudly and their new chief began a series of songs. On four occasions the women performed a dance, while the men did so once. It was of the standard variety except that the upward thrusting motion and blowing sounds were employed. In all instances, these gestures and sounds were said to encourage the spirits to depart. Near the end of the ceremony the hired chief of the Soktchum party executed a very elaborate dance again varying from the more common variety. This one featured elaborate motions of the arms and legs, almost as if swimming in an upright position. Although the public normally joins in the dancing at the conclusion, no one seemed to do so. The reasons for this failure are not clear.

The close of the ceremony was announced in three languages—Luiseño, Spanish, and English. After thanking the public for its attendance, the speaker led them in a recitation of the Nicaean Creed: "I believe in God the Father Almighty, Creator. . . . "

It is not to be doubted that all of the ceremonies being investigated in a current anthropological survey[5] of the society vary greatly from the older forms. But it is a matter of considerable interest to students of Indian culture that even these have persisted, despite the fact that for each of the surviving ceremonies no two living religious leaders perform exactly alike or know all of the appropriate procedures. However, there are reasons for believing that religious knowledge has not at any time been entirely uniform, particularly since there is a strong element of individual ownership involved. In addition there is a clear justification for this in Luiseño mythology in one of the dying acts of Wiyot. This mythical ancestor "threw away all of his knowledge," leaving his children to discover whatever each, acting individually, was able, an act which has been repeated in emulation at least twice by Luiseño religious chiefs in recent times (cf. Strong 1929: 293). Other than by individual discovery or close understudy, a new religious chief has no means of acquiring the requisite knowledge except through observation of ceremony and interpolation from custom.

But it is not only the Wiyot orientation and creation myths that provide the key to the Luiseño cultural survivals. No doubt the character of the spirits of the Luiseño dead, who linger unhappily in the vicinity is important, and the concept of a nonanthropomorphic spirit called Chingishnish, which serves to bring supernatural punishments to bear in the event of gross ceremonial failures and severe public disapproval, also requires close scrutiny in an analysis of the problem. Furthermore, the degree to which everyday life and religion were integrated in pre-Spanish times has a bearing on the problem, for strong indications of their interrelationship among the various aspects of Luiseño life still persist.

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NOTES

[1] In the vicinity of San Luis Rey and elsewhere, the image-burning ceremony, or *tochinish* (a more elaborate form alternative to the clothes-burning), has become rare, the only recent known instances having been held at Pechanga in 1937 and 1941.

[2] Harrington (in Hanna 1933: 157) lists other terms also.

[3] Kroeber (1925: 625) and Strong (1929: 300) list *tchoiyish* as being *tchuchamish*. The latter, according to my informants, refers to any kind of burning, whereas the former means specifically the ceremonial clothes-burning.

[4] The author is greatly indebted to Rejinaldo Pachito, religious chief of the Soktchum party, without whose generous assistance and solicitous desire for the preservation of all that may be salvaged of Luiseño knowledge and social structure the information upon which the present paper is based could not have been acquired.

[5] The survey is under the direction of Dr. Wm. A. Lessa to whom the author is greatly indebted for his guidance and kind assistance.

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Book Reviews

General and Theoretical

The Structure of Society. Marion J. Levy, Jr. (xvii, 584 pp., $5.00. Princeton University Press, 1952.)

This book should be read by every anthropologist. The task which it sets for itself, ". . . an attempt to construct from present knowledge of empirical materials on different societies a general conceptual scheme and theoretical system for beginning the comparative analysis of societies . . ." (p. vii) is one which has often been proposed by anthropologists as a fundamental aim of the discipline. If the result is less than entirely satisfactory, it nevertheless represents by far the most systematic and sophisticated attempt thus far to take seriously the notion of comparing total societies.

Since the book contains several hundred pages of closely reasoned argument, no adequate outline of its contents can possibly be given in a short review. The major line of argument, however, is as follows: Three approaches to comparative analysis of social phenomena are open to the social scientist. He may attempt to derive theories about "any social action," about "any social system" or about "any society." "Social action" is defined as "action . . . by individuals of a given species that (1) is explicable or analyzable in empirical terms and (2) cannot be explained or analyzed adequately . . . in terms of the factors of the heredity of that species and its environment exclusive of other members of that species" (pp. 7–8). A "social system" is "any system of social action involving a plurality of interacting individuals" (p. 19). A "society" is "a system of action in operation that (1) involves a plurality of interacting individuals of a given species . . . whose actions are primarily oriented to the system concerned and who are at least in part recruited by the sexual reproduction of members of the plurality involved, (2) is at least in theory self-sufficient for the actions of this plurality, and (3) is capable of existing longer than the life span of an individual of the type . . . involved" (p. 113). The author chooses the "society approach" because, he says, it is easier at the present stage to determine the limits of variation of societies than those of social action or social systems and because he believes that all social action or social systems must be sub-categories of societies.

Having thus defined his sphere of interest, Levy sets out to determine what statements may be made about all societies. His major conceptual tool is what he calls "structural-functional requisite analysis." A "functional requisite" is a "condition necessary for the maintenance of the unit . . . (in its) setting" (p. 62). A "structural requisite" is "a pattern . . . of action . . . necessary for the continued existence of the unit . . . (in its) setting" (p. 63). The functional requisites for *any* society are those conditions which must be present if society, as defined, is to persist. A test is provided for establishing these conditions. A condition is a functional requisite of any society if its absence would result in "(a) the biological extinction or dispersion of the members, (b) the apathy of the members, (c) the war of all against all or (d) the absorption of the society into another society" (p. 137). If society is not to be terminated in one of these ways, the author argues, the following functional requisites must be present:

(A) provision for adequate physiological relationship to the setting and for sexual recruitment; (B) role differentiation and role assignment; (C) communication; (D) shared cognitive orienta-

tions; (E) a shared, articulated set of goals; (F) regulation of choice of means; (G) regulation of affective expression; (H) adequate socialization; (I) effective control of disruptive forms of behavior; and (J) adequate institutionalization (p. 151).

The last six chapters are devoted to a discussion of the structural requisites necessary for the performance of these functional requisites in any society ("role differentiation," "solidarity," "economic allocation," "political allocation" and "integration and expression"), and to the "analytic aspects of relationship structures" ("cognitive," "affective," "goal orientation," "membership criteria," "substantive definition" and "stratification"). The influence of the thought and writings of Talcott Parsons is apparent in the terminology and is fully acknowledged by the author, who was Parsons' pupil at Harvard.

The book is packed with sociological wisdom. Levy clearly has thought about and tried to find answers to nearly every major problem in the field. Nevertheless, it is just in the area which he has chosen as his main concern, the systematic comparison of whole societies, that the book seems least satisfactory. If one is to compare whole societies, one must first find some examples to work with. The thorniest problem arises in connection with the second point in the definition of society: "(a system of action which) is at least in theory self-sufficient for the action of (the plurality of actors involved)." On a narrow interpretation, one might limit the number of existing "societies" to a handful; if one were less strict, thousands might be found. The author recognizes this problem and mentions (pp. 133–34) two difficult examples: occupied Japan and American Indian reservation communities. In neither example, however, can he give a clear "yes" or "no" answer. Surely, if the limits of uncertainty in the definition of the basic unit for comparison are so broad, it seems difficult to imagine systematic results emerging from its use! Similar problems arise at the next stage, the determination of the functional requisites of any society. Many of the requisites appear either to refer to biological requirements for survival (functional requisites for "any social action" whatsoever) or else to be variable within extremely wide limits.

The present volume is concerned mainly with establishing the functional and structural requisites of any society, but it is clear that the author's ultimate aim is to determine the structural and functional requisites of particular types of societies. At this point, as the author recognizes, one is led immediately into the problem of social change, since one of the major ways of determining whether or not a given concrete structure is "eufunctional" (making for the persistence of the unit) or "dysfunctional" (making for lack of persistence of the unit) is to observe whether or not its presence or absence results in other structural changes in the unit. The mere presence or absence of a concrete structure cannot be taken as evidence of its eufunctional or dysfunctional character since to do this, the author recognizes, is to commit the logical fallacy of "functional teleology." The difficulty is, however, that it is just in this area that social science knowledge is at present weakest. We have very little real knowledge about the closeness of structural-functional articulation of various types of concrete structures because we as yet know little about the limits of variation in actors' ability to occupy conflicting roles simultaneously. Levy might argue that it is just such knowledge that his conceptual scheme will help us to acquire. But it would appear to this reviewer that such knowledge, which ultimately must underlie work on the macroscopic level of the

sort Levy proposes, is more fruitfully sought not by the gross comparison of whole societies, but by the study of the problem of role integration in more circumscribed systems of action ("any social system"). The "society" concept is useful because it prevents us from forgetting the setting in which any sub-system exists, but it seems premature to use it as the major unit of comparison while so much of the necessary knowledge about social systems in general is still lacking.

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A Hundred Years of Anthropology. T. K. Penniman. (512 pp., revised edition, $5.00. Gerold Duckworth & Co., London, 1952.)

Except for Chapter VI, this is a reissue of the first edition. Leslie White reviewed the latter in 1937 (American Anthropologist 39: 157 ff); his characterization of it at that time stands almost intact.

When the book first appeared (1935), the title was unhappy: Dr. Penniman was unable to make the artifact 1835 a landmark in anthropological history. With almost two decades added thereunto, all vestige of meaning in the title vanishes. The point would not be worth mentioning but that it symbolizes a larger condition. No historian would call this book a history. It is, rather, a chronology of a sort. This reviewer had the sensation of filing past show-cases in a museum of anthropological literature, carrying a guide-book—a useful thing, to be sure, under the circumstances.

The reproach is less true of the earlier part of the original book, than of the later parts; yet the closer it approaches the contemporary, the more its perspective deteriorates. Latterly, more and more titles of books, with annotations about their significance plus authors, troop in to crowd out the last chances of a binding narrative. Not very wakeful reading even for the anthropologist, let alone the layman whom the book obviously hopes to capture.

The new edition adds some American show-cases, and houses them in a separate hall. This is an improvement for the first edition was hardly aware that Americans existed. At last there is mention of Kroeber, Lowie, Swanton, Densmore et al. How satisfactory the addition is, must depend upon whether the reader judges that Betty Meggers did a good job in her "Recent Trends in American Ethnology" (American Anthropologist 48: 176–214), for on that article Penniman relies heavily.

This addition is part of the appendage, Chapter VI, which also includes: Prehistory and Technology in the Old World; Physical Anthropology since 1935 (by J. S. Weiner —the most satisfactory part of the addition); Americanist Studies (by Beatrice Blackwood; a rather fair survey, though already outdated); and General Ethnology and Social Anthropology. There is no special account of Africanist studies to balance the American. The impression remains that anthropology has been pursued almost exclusively by speakers of English, German, French, Italian. The first edition ignored linguistics after its initial years, and so missed its most fruitful age; the new edition adds a half-page which would not be missed if it were to be dropped. We might have expected that the author would have spoken golden words in that realm where he himself has been so productively active: Levantine archeology; the more especially as

discoveries like those of Ras Shamra have well-nigh revolutionized certain aspects of culture history; but no.

In 1935, Dr. Penniman undertook a very much needed thing. It was a brave, a useful, a lonely attempt. Anthropology itself is a movement in culture: who else should know this better than anthropologists, and be better equipped to show it? In the howling and stormy mid-twentieth century, with at least a century and a half of systematic attempt to understand man now behind us, we need the perspective which a historical study of a discipline could give us. In 1935, Dr. Penniman did not give us what the title of the book might have led us to expect; but since then, has there been no opportunity to rewrite completely, taking cognizance of earlier lacunae; to integrate important materials hitherto quite omitted, and bring out a pattern that is there to be seen; to focus perspective and write history as history is now being written?

But the reflection is far less upon him who tried and did not succeed than upon the cultural perspective of a discipline which perforce must offer this book as still the best account of itself in the English language.

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ETHNOLOGY AND ETHNOGRAPHY

Kinship and Marriage among the Nuer. E. E. Evans-Pritchard. (xi, 183 pp., $3.00. Oxford University Press, New York, 1951.)

The appearance of this volume is a major event for students of social structure and Africanists alike, despite the fact that large portions of it are reproduced almost verbatim from previously published papers.

In the first chapter the composition of the community and the bonds of kinship between its members are described in concrete detail. "A Nuer community, whether small or big, is not composed exclusively of members of a single lineage and their wives or, correspondingly, not all members of a lineage live in the same community." (p. 23) Although the reader might have welcomed a discussion of the implications of this type of local community for the lineage system in contrast, say, with the Zulu, where the minimal lineage is coordinate with the local group and clearly a corporate unit of a very different sort from the Nuer, such discussion is lacking. Instead we learn that ". . . it is the clear, consistent, and deeply rooted lineage structure of the Nuer which permits persons and families to move about and attach themselves freely . . . to whatever community they choose by whatever cognatic or affinal tie they find it convenient to emphasize . . ." (p. 28), and "It would seem it may be partly just because the agnatic principle is unchallenged in Nuer society that the tracing of descent through women is so prominent and matrilocality so prevalent." (p. 28) Can we conclude from this that the agnatic principle is *not* so deeply rooted among the Zulu and is somehow challenged, since their kraals are composed primarily of agnatically related kin and only rarely if ever do the Zulu live matrilocally?

The second chapter details marriage prohibitions and the Nuer definition of incest. The conclusion is interesting: "Nuer say that marriage to persons standing in certain relationships is forbidden because it would be *rual*, incestuous. Speaking sociologically, I think we may reverse this statement and say that sexual relations with persons stand-

ing in these relationships are considered incestuous because it would be a breach of the marriage prohibitions to marry them. I would hold that the incest taboo can only be understood by reference to the marriage prohibitions, and that these prohibitions must be viewed in the light of their function in the Nuer kinship system and in their whole social structure." (pp. 43–44) Whatever "sociologically speaking" (p. 43) may mean, the problem is hardly settled by this assertion alone. What perversity accounts for the fact that the Nuer state the relationship the other way around? Why is it that "the incest taboo is not uniform in its force nor in the consequences its breach is believed to entail" (p. 40), yet the prohibition on marriage is uniform within some of these same categories? Why, that is, is incest between mother and son "worst of all" while incest with a paternal half-sister is bad, but not so bad, when marriage with either is equally prohibited? That incest prohibitions are prohibitions on sexual relations and that these are something else than marriage is recognized and stated at the outset. (pp. 29–30) But it has never been made clear why sexual relations must be prohibited where marriage is prohibited, despite the fact that under certain circumstances sexual relations can and do occur where marriage is prohibited, and this does not lead to the collapse of the prohibition on marriage. Given these questions it is difficult to understand how ". . . the incest taboo can *only* [reviewer's italics] be understood by reference to the marriage prohibitions . . ." (p. 44). No one will deny that the incest taboo is one of the strongest sanctions supporting marriage prohibitions, but to suggest that this exhausts the problem is hardly worth serious consideration today. For this still leaves one crucial question: If the incest taboo is the sanction supporting marriage prohibitions and its ". . . range of extension may be related to the marriage prohibitions . . . for which it provides a moral norm, a reason, and a sanction" (p. 44), how does it happen that the sanction is breached more often than that which the sanction supports, namely, the marriage prohibitions? For unless this reviewer's reading of the Nuer evidence has been grossly distorted, incest occurs more frequently than prohibited marriages (especially pp. 39, 40, 41, 44 and 45).

The third chapter opens with an account of courtship and the wedding, and proceeds by detailing the complex distribution of cattle which legitimate a union. This is followed by a discussion of the function of bridewealth, the conclusion of which is that "The payments [of bridewealth] do not so much give stability to the union of marriage as they are a recognition of its stability." (p. 96) This contention merits careful consideration for, despite the force and eloquence of the argument, it proves untenable. Limitations of space preclude its detailed consideration here. The chapter is concluded by a brilliant and by now classic account of the various forms of the family and the different kinds of conjugal union which obtain among the Nuer.

The two concluding chapters, The Family, and Kinship, detail the nature of relationships between various categories of kin. The structured strains within the family are sensitively traced and balanced by the discerning illumination of bonds of firm and enduring solidarity. The suggestion seems to be implicit that the organization of the lineage system is patterned after the structure of the compound family, yet the moral nature of the family, consequent on the incest taboo, and its small size appear to preclude its persistence in time as a corporate structure. The reader would have been enlightened by explicit consideration of the question of the relation between the structure of the polygynous family and the lineage system.

Although there is a disquieting tendency both in the explicit theoretical sections and in the apparently purely descriptive portions to frame balance and imbalance in terms of polar opposites each of which is the direct reversal of the other, the studied simplicity of style mutes the dissonance. Nevertheless, each explicit theoretical point which is thus framed appears, on close examination, to be dubious. Thus, for instance, the Nuer say that marriage of certain persons is forbidden because it would be incestuous but Evans-Pritchard says that it is incestuous because marriage is forbidden; "It is therefore a common-sense inference that payment of bridewealth has a stabilizing action in marriage." (p. 90) But after looking into the question it appears that "the payments do not so much give stability to the union of marriage as they are a recognition of its stability." (p. 96) Tracing descent through women and matrilocal residence are so prevalent not because agnatic ties are at times weak or not relevant, but just because they are so strong and deeply rooted. (p. 28)

To list the values of this book, despite the few points noted above, would require far more space than is allowed here, and this is the true measure of its worth. It is indispensable to the student of social structure because it complements Evans-Pritchard's classic study of the lineage system; the chapters on Marriage and The Family and the chapter on Kinship will remain classics so long as anthropologists are interested in kinship and the family; it is a mine of fruitful and stimulating theoretical questions.

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The Hall of Light; a study of early Chinese kingship. William Edward Soothill. (Ed. by Lady Hosie & G. F. Hudson. xxii, 289 pp., $7.50. Philosophical Library, New York, 1952.)

When he died in 1935 Prof. Soothill, long noted for his contributions to the study of Chinese history, literature and religion, left upon his desk substantial portions of a work in which he had been engrossed for many years. That manuscript, now edited by G. F. Hudson and Soothill's daughter, Lady Hosie, has been made available as *The Hall of Light* with the imposing sub-title, "A Study of Early Chinese Kingship." It will be read with great interest by those who are familiar with Max Weber's analysis of Chinese religion and politics, and by persons with interests, pro or con, in the hydraulic approach to Chinese history.

Despite the promise of the title and the assurances of the author that "ultimately, of course, it will be the Chinese archeologist and anthropologist . . . who will bring the most welcome contribution," (p. 22) it will interest anthropologists that Soothill's model in this discipline was Sir James Frazer, and that the bases upon which comparisons of institutions are made seem fairly random. Thus, the analyses which appear in *The Hall of Light* bearing on the question of the origin of Kingship represent to the reviewer a detour rather than a main thoroughfare of inquiry in this problem. Indeed, if he is not exceptionally versed in classical Chinese scholarship, the anthropological reader will find this volume quite prolix in matters that are of secondary concern.

But, of course, Soothill wrote as a sinologist and for sinologists, despite his interesting attempts to bring a broad anthropological approach to the study. The "Hall of Light" was the *ming t'ang*, the building or group of buildings at which, according to

diverse sources, the royal observances of calendrical rites were celebrated. Taking his cue from the description of this institution which is to be found in the *Book of Rites* (*Li Chi*) and utilizing two important 18th century Chinese works on the subject, as well as a wealth of other data drawn from diverse Chinese and western sources, Soothill reconstructs the physical apparatus, the ritual and the significance of the *ming t'ang*. His thesis may be paraphrased as follows. "Chinese" culture is not homogeneous in its origins but represents the fusion of many tribal traditions, such as the Jung, Miao, T'ai, etc., into a unified civilization dominated by one of the constituent traditions. The dominant tradition is Chinese. It attained this central position through the power of its religious organization. In Soothill's words, it "derived its power from the religion, ritual, and sacrifice, which welded the tribes together under a priest-king possessed of superior knowledge, mimetic potency, and a calendar." (p. 228) Since the major aspects of the economy rested upon a control of the knowledge of calendrical data and since that control was primarily manifested in the *ming t'ang* complex, the crucial importance of the study of the institution is manifest.

Acceptance or rejection of Soothill's explicit reconstructions can only rest with competent sinologists. The reviewer, however, does not relinquish the duty of indicating some of the weaknesses in Soothill's anthropology. As previously remarked, the over-all tone of the anthropological approach is nineteenth century, and not Morgan or Tylor so much as Frazer. More specifically, one might note such remarks as "primitive magic is but another name for primitive science" (p. 67), or the more obviously incorrect, " 'spiritual' life was confined, as is indeed usual today amongst primitive peoples, to . . . material needs. The soul, wonder, awe, craving for eternal life, spiritual exhaltation could have been matters of little or no knowledge to him." (p. 170)

Prof. Soothill's last book will most interest those anthropologists whose attention has been engaged by the hypotheses on "oriental society." It will also reward those who are working on the relationship between religious and political institutions, and is a must for those who would pursue the elusive study of the origin of the State.

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India: A Short Cultural History. H. G. Rawlinson. (xiv, 454 pp., 24 plates, 45 text figures, 12 maps, $8.00. Frederick A. Praeger, New York, 1952.)

This attractive volume should appeal to the general reader, for whom it is designed. It should certainly impress him with the diversity of geographical features, peoples, languages, and religions to be found in India at all stages of its history. It should also encourage him to delve into more detailed treatises.

To the student already familiar with Indian history, however, Rawlinson's work will seem to cover well-worn ground. This in part is because of the limitations put on illustration and quotation in the scope of 400 pages. It is also due to the fact that very little history as such was written in India prior to the arrival of the Moslems in the 12th century A.D. The Vedic and Buddhist works and the foreign sources upon which historians depend are so few and so well-known as to provide the author with small scope for variety.

Certain passages in this book would make any archeologist's mouth water. On the

other hand, the social anthropologist, who would probably be seeking to understand Indian village life, would be disappointed in it. Early Indian social organization and customs have to be interpreted from passages in religious literature whose aim was not the documentation of history or social custom. The *Rigveda*, the Hindu epics of the *Ramayana* and *Mahabharata*, and the Buddhist *Jatakas* are full of information, but they need to be read at length.

For later periods, Indian history is primarily that of kings and conquests. Kautilya's *Arthasastra*, which may date from the 4th century B.C., Megasthenes' account of India, written about 300 B.C., the Laws of Manu, possibly belonging to the Gupta era near the time of Christ, and the *Ain-i-Akbari* or Institutes of Akbar, the great Mogul emperor of the 16th century A.D., are notable exceptions. These cannot be treated satisfactorily in one volume, however.

Furthermore, Indian history from the earliest times to the present is that of urban civilizations. Harappa and Mohenjo-daro of the third millenium B.C. were large cities, whose story is still hidden in undecipherable script. Megasthenes described the capital of Chandragupta Maurya as nine miles long by two miles wide, inside the walls were inns, gaming houses, theatres, race-courses, guild halls, religious edifices, a royal palace, and a park. The Chinese pilgrims Fa Hien and Hiuen Tsang, who toured India in the 5th and 7th centuries A.D. respectively, described similar cities of great wealth and with a flourishing international trade. In the 15th and 16th centuries A.D., Italian, Portuguese, and Afghan travellers were hard put to find adjectives properly illustrating the richness of the capital of the Vijayanagar empire in south India.

The descriptions of Indian culture accompanying Rawlinson's historical narrative are largely confined to art, architecture, and religion. The student searching for data on the development of caste, for instance, will not find it except incidentally. For historical ethnography, he will be driven to more complete works which, though rewarding, are not to be undertaken lightly. Indian village culture, aside from that in primitive tribal areas, still has to be studied mainly in the field. In general, therefore, Rawlinson's cultural history of India can serve only as an elementary introduction rather than as a source-book of information for the anthropologist.

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CULTURE CHANGE

Menomini Peyotism. J. S. Slotkin. (135 pp., $2.00. Transactions of the American Philosophical Society, vol. 42, part 4. The American Philosophical Society, Philadelphia, 1952.)

Slotkin has made a significant contribution to the literature on the Peyote cult. Because of the manner of collecting and presenting his data, as well as for the type of information obtained, Slotkin's report is a most welcome addition to our knowledge of the American Indians' most vigorous contemporary intertribal religion. The major part of the monograph consists of transcriptions of recordings of statements made by Indians in English or of translations of similar recordings of statements in Menomini. By using a recording machine to preserve exactly both questions and answers and by publishing the interview material as transcribed, the report has a completeness not

previously obtained. In addition, informants' statements preserve the simplicity characteristic of people slightly educated and using a foreign language. Its very repetitiousness gives it a charm and an appearance of authenticity. It would be difficult to doubt the strong and sincere religious convictions of the Menomini Peyotists after reading this monograph. Furthermore, Menomini Peyotists seemed to me to express the same feelings in about the same way as did Ute and Northern Paiute Peyotists I have talked with.

Slotkin judged Menomini Peyotism to be similar in ritual and theology, including acceptable variations, to that of the Native American Church, which tries to embrace all Peyotists. "Essentially," Slotkin wrote, "the religion is Christianity adapted to traditional Indian beliefs and practices." The several verbatim testimonies and the exact responses by different informants to questions regarding the meaning of various aspects of the cult are most valuable to substantiate Slotkin's judgment.

An appendix of twenty pages on Menomini Peyote Music by David P. McAllester provides additional evidence that Menomini Peyotism fits into the general intertribal pattern of the Native American Church.

Dr. Slotkin was invited to produce a document on the Peyote cult by the Menomini devotees, and thus he had nearly ideal conditions for this field research. This hardly justifies, however, the American Philosophical Society's saying in the publicity sheet announcing his monograph that "this is the first study made of the American Indian Peyote Cult which is based upon intimate knowledge of its inner workings by an anthropologist who participated in the cult as a member." Probably a dozen anthropologists, all who attended more than one meeting, were accepted as members of the Cult before Slotkin was admitted into Menomini Peyotism.

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PHYSICAL

Les Hommes Fossiles, Éléments de Paléontologie Humaine. Marcellin Boule and Henri V. Vallois. (x, 583 pp., 299 figs., fourth edition. Masson et Cie., Paris, 1952.)

Boule prophesied without chagrin that the first edition of this book would be out of date as soon as it was printed, and this is only part of the hazard that is faced by anyone wishing to publish a compendium of fossil men; new finds call not only for insertion, but also for re-evaluation of older finds. Small wonder that *Les Hommes Fossiles* nowadays stands alone in the field; and it is fortunate that Vallois, Boule's successor in this as in other affairs, has seen fit once more to bring it up to date. It is really a good book and not just a catalogue. It is written with grace and gusto, particularly when dealing with ancient Frenchmen, and it keeps a most successful balance, for its purposes, between fact and opinion. Its principal emphasis is on description, not only of the specimens, but also of their discoverers and discovery, a kind of treatment that is not as common as it might be in non-technical surveys of early man. It should be useful to general anthropologists both for reference and for teaching. This does not mean that there is not a wealth of opinion supplied as well. The final summary of 28 pages is rather discursive and speculative, but there are sections of conclusions at appropriate places all through the book: it is not always possible to disentangle Boule and Vallois,

but both shared a skepticism and a preference for concrete evidence which has probably resulted in calling for less revision from edition to edition than might otherwise have been necessary. Boule, for example, would have no truck with eoliths or such supposed ancient sapiens as the Ipswich skeleton, and he never even bit on Galley Hill; Vallois rejects equally firmly all other such dubiously documented cases.

The fossil men are dealt with as follows. Pithecanthropus and Sinanthropus are the "prehominiens," cranially intermediate between anthropoid apes and man. In Europe there are the men of the Lower Paleolithic: first the Heidelberg jaw; then the Preneandertals (e.g. Ehringsdorf, Saccopastore, Steinheim), and the Presapiens. These are led off by Piltdown; Boule believed the jaw was an ape's, but Vallois, convinced by the flourine content of the parts, considers that they go together and possibly form the base, as now known, of the sapiens line, and that they deserve the designation *Homo* rather than *Eoanthropus*. This line is continued by Swanscombe and Fontéchevade. The Neandertals proper of the final phase were "doubly fossil," an archaic survival even then, with no modern descendants in the proper sense. The men of the Upper Paleolithic and Mesolithic are described in detail. The Grimaldi pair were indubitable negroids, and the Chancelade man, while certainly no Eskimo, did suggest the "yellow" a little.

This is a system for western Europe, and the other fossils are treated more geographically than systematically, which will probably displease those looking for broad theoretical discussion more than it will the general reader. The Rhodesian and Solo forms are treated briefly (and as races of Neandertal man). Proconsul gets short shrift—a few lines—and the Australopithecinae are also given relatively few pages. Furthermore, the whole australopithecine nettle has not been grasped very firmly and seems to leave the authors pricked: early in the book the Australopithecinae are "far from being our ancestors" because, in spite of teeth and posture, the brain was still that of an ape; toward the end, the man-apes could be placed in our direct ancestry were it not for their apparent recency, because of their uprightness. I think this lack of resolution is one result of the two different degrees of intensity with which the European fossils on the one hand, and most of the rest on the other, are treated. But that is certainly the authors' privilege. It is evidently not the purpose of the book to argue any final or detailed reconstruction of human history, but rather to present the facts for each human type, together with those hypotheses which have been offered to explain them, among which the authors make their choice and the reasons therefor. Certainly this is what a general book, as opposed to a monograph, calls for, and *Les Hommes Fossiles* could hardly be more successful in its purposes.

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Archeology

Measurements of Some Prehistoric Design Developments in the Southeastern States. James A. Ford. (75 pp., 23 figs., Anthropological Papers of the American Museum of Natural History, Vol. 44, Part 3. New York, 1952.)

Ford's paper deals with the relative chronological position of a considerable number of archaeological pottery assemblages. In nearly every case the assemblage is a collection

of potsherds found in an arbitrarily defined vertical component of a test excavation in refuse deposits or in a stratum resulting from two or more definable phases of earth mound construction. The chronological positions assigned are said to be inferences derived from (1) the evidence of superposition, (2) an application of the widely accepted proposition that two assemblages which resemble each other closely are not far removed in time, and (3) an evolutionary ranking of certain ceramic attributes and attribute combinations in terms of ancestral and descendant types. A considerable part of the publication is devoted to the closely related problem of determining the point of first appearance (within the area considered) of certain attributes, and the direction and relative speed of their diffusion together with concomitant changes in their physical expression. The term "measurement" presumably refers to use of pottery type percentages; although the word appears a number of times in the text, I was unable to find a coherent statement of what was measured with respect to what scale. It appears, however, to be employed on occasion roughly as an equivalent for counting and ranking, a decidedly misleading practice.

No effort will be made to summarize and discuss specific results, on the ground that the study reveals such serious methodological deficiencies as to make an appraisal of the reliability and implications of the conclusions unprofitable. Under these circumstances, the reviewer's task must be an attempt to validate his judgment by describing and illustrating the putative defects. Since the errors are primarily fundamental misapprehensions of scientific method rather than narrowly technical slips, they are of general interest.

Two characteristic examples can be drawn from the Introduction, which is intended to show the general position of archaeology with relation to cultural anthropology. The first example (pp. 317–318) is the statement:

> However, archaeologists have perhaps less excuse than students in any other field to be blind to the fact that not only the methods but the ultimate objectives of their discipline are slowly but inevitably changing. One of the present less-popular concepts of the purposes of archaeological study will become the objective of the majority and then historiography pursued for its own sake will be old-fashioned. Of course, any new objective will in turn be superseded, but we probably cannot now imagine in what way; neither can we be too concerned with this nebulous cultural type that will evolve from a stage of this discipline that is not yet fully developed. It is sufficient to recognize that our study is merely another example of cultural phenomena and to align our activities in the direction of the historical trend. To endeavor to exceed the limits of sound methodology and information is quite as unrealistic as to lag behind. Dissipating effort in some divergent bypath destined to be abandoned is a waste of time. The most productive position is one slightly in advance of the majority.

This statement fails to recognize the fundamental principle that we possess a method of investigation which will disclose real truths (or approximations to real truths) about a real world if it is properly applied. The fact that this method is of cultural origin is inconsequential as a guide for action. Soundness of archaeological methodology and productivity can be gauged only in terms of additions to knowledge. The ultimate objective of archaeology is immutable—it is to achieve a systematic interconnection of facts within the field of archaeological data, and of archaeological data to all other data. This is, of course, an ideal toward which the archaeologist struggles by successive approximations, but it is one which must be kept firmly in mind if archaeology is to be

maintained as an intellectually respectable pursuit. The only changes which can occur are those connected with immediate objectives within the framework of this general purpose; they are the result of the continuous process of observation—hypothesis—new observation. Ford's propositions carry the logical implications that truth is to be determined by some sort of polling of archaeologists, that productivity is doing what other archaeologists do, and that the only purpose of archaeology is to make archaeologists happy. This is simply a specialized version of the "life is just a game" constellation of ideas, a philosophical position which cannot be tolerated in a scientific context.

The second example (p. 318) of defective reasoning is the ascription of logical priority to archaeological studies:

> Every living culture is composed of elements inherited from the past and modified. To attempt to describe and analyze any culture without this background resembles the description of a mountain range without reference to historical geology: it is art, not science.

One of the implications of this statement is the notion that limited objectives cannot be pursued in a scientific manner, disheartening news to topographers, structural geologists, linguists, ethnographers, and indeed to any working scientist, since the act of working automatically imposes limitations. Descriptions and analyses which are consciously and systematically less than ideal are a fundamental feature of scientific research. A second objectionable implication is the dictum that the present can be understood only in terms of the past. In point of fact, all our knowledge of the past consists of inferences based on observation of current processes and on present material interpreted as relics of the past, a situation which effectively precludes any claim of special priority for historical studies. Historical inferences provide no more than a part of the data required for the understanding of currently existing culture and are indispensable only in the sense that an ideal description demands the marshaling of all relevant evidence.

Similar flaws in reasoning are at least as prevalent at the level of specific problems. An important part of Ford's research consists of ordering components on the basis of their degree of likeness, on the assumption that such an ordering will reflect closely or exactly their chronological rank. The units of likeness are potsherds exhibiting a specified combination of attributes (a "pottery type" in the customary terminology of American archaeology); for ordering, the potsherds of a given type from a component are counted and the total is divided by the total of all potsherds to give a percentage expression of the frequency of the attribute combination. A component can then be represented by a histogram, the individual bars showing on a percentage scale the relative frequencies of each of the pottery types noted, and the histograms of a group of successive components drawn from one cultural tradition can ideally be arranged on a chart so that the bars for each pottery type form a smooth unimodal curve. There are, of course, a large number of arrangements possible if many components are to be ordered, although in practice most can be eliminated at once as absurd. A second feature of practical work is the fact that perfect arrangements are never possible, presumably owing to such factors as inadequate samples, multiple occupations, disturbed deposits, errors in typology, geographical variation, and so on. In Ford's case, arrangements were made entirely by graphical methods, with the result that he can only assert that his final ordering is the best possible under the circumstances and reproduce the graphs to substantiate the assertion. The absence of any mathematical expression of degree of fit

leaves the skeptical reader with no recourse other than reproducing the component histograms and trying new arrangements himself, a task which is made difficult by the absence of the original counts on which the percentages were calculated.

A far more serious objection to Ford's attack on the problem can be made, however. This objection is, briefly, that no effective consideration is given to negative data. The numerous discrepancies in the lengths of percentage bars are dismissed on the basis of sampling error, disturbed deposits, and the like, but no attempt whatsoever has been made at an independent classification of components in terms of adequacy of sample or presence or absence of disturbance. This substitution of *ad hoc* explanations for systematic classification is not in accord with basic scientific principles and results in a hypothesis lacking in logical implications and hence incapable of being tested. The general impression given by the presentation is that the true situation is known on the basis of evidence not available to the reader, and consequently that the cause of any particular discrepancy is of no great importance and can be treated in an offhand manner.

An example of this tendency is the treatment of certain data from the western part of the Florida Gulf Coast (Figure 4). Here, in accordance with Ford's standard practice, each set of bars representing a pottery type is fitted with a "smoothing curve" purporting to show the unimodal trend of percentage frequencies. It appeared on inspection that the smoothing operation was more in the nature of a bloody amputation. This impression was checked by computing Spearman's Rank Correlation Coefficient for each type and then calculating Student's t to test the significance of the correlation coefficients. Half of the calculations of t resulted in probabilities between .50 and .10 (and in one case of more than .50), which indicates that very often no particular relationship exists between the smoothing curves and the data which they are supposed to represent. Although it can be granted freely that these calculations by no means exhaust the subject, they are sufficient evidence to establish the existence of a very serious problem ignored by Ford, especially when it is realized that the smoothing curves and not the histograms are used in drawing chronological inferences. In this connection, it is interesting to note that all possible arrangements of a four bar histogram show a significant trend if one bar is allowed to be out of place, i.e., they must show an ascending order, descending order, or a peak if the bars are of different lengths; furthermore, either an ascending or a descending order will appear by chance alone 9 out of 24 times and a peak will appear in 16 out of 24 trials.

Other examples of inadequate or erroneous method could be cited, but it is more important to consider the general problem raised by Ford's publication: to ask how these errors could be committed in a field which is scientific in intent. Ford is an energetic and respected archaeologist with a detailed knowledge of southeastern prehistory. Many of his interpretations may prove to be correct, but his methods of analysis and presentation deprive him of the opportunity to prove their correctness. The principles he uses are well-recognized and can be employed to yield valid results. Perhaps there is too much pressure for immediate results. It can only be hoped that careful evaluation of Ford's work will serve as a corrective and demonstrate to all archaeologists the necessity for rigorous methodology.

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Archeology of Eastern United States. Edited by James B. Griffin. (x, 392 pp., 205 figs., $10.00. University of Chicago Press, Chicago, 1952.)

Archeology of Eastern United States, consisting of papers by twenty-seven authors, all but one of whom is a former pupil of Fay-Cooper Cole, was "prepared in his honor and appreciation of the part he has played in the study of Eastern United States archeology." The volume has been eagerly awaited since 1946 or 1947, when word of its preparation first got about. Most of the chapters were prepared in 1947, some in 1948, one in 1949, and one in 1950; many were partially revised in 1951. Lag in publication has always dogged archeological writings, usually without seriously affecting the work in press. In this case, however, the interval between submission of manuscript and publication of the volume covered a period of very great activity in archeology. As a result of increased knowledge and a change in point of view occurring in this time, much of the archeological theory in this book has become obsolete; certainly the chronological framework, except for a few "ringer" dates inserted after manuscripts were submitted, has been outmoded by dates obtained by the radiocarbon method. For this reason if for no other it is especially difficult to judge the book fairly. In spite of the fact that so much can no longer be accepted as valid, the volume is, and will continue to be an important milestone in archeological history.

A detailed review is being prepared for publication in *American Antiquity*. The present review is concerned only with fundamental archeological philosophy and the problems of preparing such a book.

A volume such as that under consideration offers a golden opportunity to put before the general public all of the things which we as archeologists like to think that we have accomplished. In order to fulfill this promise and produce an integrated whole, editorial planning must extend beyond decisions regarding style into the very substance of the book. It seems to this reviewer that the volume lacks integration of philosophy, of approach, and even of the fundamental concept of the aims and purposes of archeology.

By 1947, when most of the chapters were prepared, the archeology of the eastern United States was assuming depth and perspective; most important of all, a breath of life was beginning to waken the dust. Although some workers were striving to reconstruct the life of people, others still seemed to aim only at classifying things—artifacts, potsherds, or "cultures"—into categories which probably bear no relation to vital circumstances of aboriginal life. While such a step is necessary for proper analysis, it is by no means the ultimate objective of archeological research. All too much of the book under review is concerned with taxonomy, and with the listing of taxonomic subdivisions which lack shred of flesh or drop of blood. These portions are a most effective soporific.

Of the twenty-nine chapters, including a hurriedly added appendix listing some radiocarbon dates, twenty-six are concerned with telling the story of the Indian. Not only is the story divided among twenty-six authors, but States Rights have been considered. As a result, there is no connected story of life along the Tennessee River, for example. One is forced to consult several different chapters in order to sort out what is desired from among other material. Similarly, the Hopewell movement is divided among so many different compartments that it is practically impossible to fit together a proper picture of what was undoubtedly one of the great religious expressions of pre-

historic North America. The other, the "Southern Cult," is similarly fragmented.

One gathers an impression that prehistoric America was inhabited by "cultures" consisting of pots and burial habits; a world in which humans were unnecessary. We read, on page 164, of "villages in which two, or more, different cultures cohabited."

It is evident that contributors are aware of the shortcomings of an archeological structure reared primarily on potsherds and taxonomy. John Griffin (pp. 332–333) laments "that these sequences are for the most part based on ceramics," yet he only mentions the striking Key Marco site in passing. Coe expresses a view (p. 303) that the introduction of pottery-making marks the attainment of a certain cultural level, rather than a point in time, a tenet that is not too clearly seen through swirling taxonomic clouds. Fairbanks (p. 297) notes that both Muskhogean-speaking Creeks, and Iroquoian-speaking Cherokee were subscribers to Lamar standards, and concludes that setting up a Lamar Aspect "would be largely a study in virtuosity . . . and the addition of aspectual and focal terminology would simply becloud the issue." Jennings bemoans the emphasis on pottery and points out (p. 271) that there is a "coherent cohesive core of cultural elements which color and influence all cultures of the South, and . . . that the true story of the region is a story of accretion and enrichment of this basic core." John Bennett (p. 111) states that "the question of distinguishing between Hopewellian and non-Hopewellian manifestations and traits has become confused, largely because of the persistence of the older mindset which viewed Hopewellian as *a specific historical culture*, instead of a horizon or style within a certain general period of the development of Woodland culture."

Recognition is due Moreau Maxwell and Madeline Kneberg for their really excellent chapters. Both authors emphasize continuity of basic culture with successive increments of new traits. Their description of backward groups surviving in out-of-the-way areas demonstrates the fallacy of the concept of "culture periods."

Americanist archeology has a thrilling story to tell, a story in which the general public will be greatly interested. *Archeology of Eastern United States* was obviously aimed at a portion of that public as well as at archeologists. Unfortunately many sections of the book are couched in that esoteric jargon which is not intelligible even to archeologists who are not initiates. As a result, the greater part of the book is directed toward a relatively small number of specialists, and it misses an opportunity to be of much broader scientific interest and value. As a compendium of details with schematic pictorial presentation of data, the book will be of more use to the public than anything published within recent years. The addition of clear and truly interpretive sections would have increased its appeal and its capacity to serve the public to whom we must all turn eventually for backing.

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Urnas de Oaxaca. Alfonso Caso and Ignacio Bernal. (389 pp., 527 figs., Memorias del Instituto Nacional de Antropologia e Historia, II, Mexico, D. F., 1952.)

This is a major publication resulting from the long series of archeological studies in the State of Oaxaca begun by Alfonso Caso in the 1920's and continued ever since by

an able group of Mexican scholars under his direction. The urns form one of the most important classes of antiquities from the region. They are the often very large and elaborate clay sculptures attached to vessels, usually gray in color, and usually labeled in the many museums where they are exhibited as *funerary* urns or *Zapotec* urns. Such descriptive terms are omitted from the title of this book, however, for the authors state that these objects were not used exclusively for funerary purposes and also that they cannot certainly be ascribed only to Zapotec-speaking peoples.

We have long needed a thorough study of the Oaxaca urns such as we now have available in this superb monograph. It has been obvious that their complex forms contained an extraordinarily rich source of information on Mexican religious beliefs and practices, but it has been obvious too that their analysis would not be a simple matter. It could have been done adequately only by persons like Caso and Bernal who have an exhaustive knowledge of the archeology of Oaxaca as well as of the archeological and documentary history of all other portions of Mesoamerica. With their unique knowledge and their unusual flair for the interpretation of complex phenomena they have produced a volume which is undoubtedly the most important contribution to the study of Mexican religions, art, and related subjects that has appeared in many years.

The book is arranged according to a classification of the urns into the various deities or personages represented. For each grouping one or more typical examples are first described and this is followed by a discussion of those less easily identified. Finally all available evidence for the chronological placement or the sequential development of a type is presented, principally on the basis of the excavations at Monte Alban. This treatment of the more readily classifiable examples is followed by a section containing the unusual or unique pieces identified, where possible, according to period. The last chapters of the book are concerned with the few literary sources on the gods of the Zapotecs and an outline of the sequence of appearance of the various members of the pantheon according to the archeological chronology. The entire organization of the book shows the authors' interest to be somewhat more in the direction of the meaning or the content of the sculptures than toward a mere chronological ordering of them. To my way of thinking they have achieved a very sound balance between these two basic aspects of archeological research.

The study is based primarily on the collection of urns in the National Museum of Mexico and in that of Oaxaca as well as on private collections in Mexico, although reference is often made to published specimens in other museums. A really formidable series of illustrations is included, in both half-tone and in line cuts. These comprise 527 numbered figures, but since many of these figures include two or more pieces the total number of objects illustrated must run to nearly a thousand. With the entire book printed on coated paper, the illustrations are incorporated within the text in a manner making for remarkably easy reference. The printing of text and pictures is excellent and there is a most useful map forming the end papers of the book.

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Mogollon Cultural Continuity and Change, The Stratigraphic Analysis of Tularosa and Cordova Caves. Paul S. Martin, John B. Rinaldo, Elaine Bluhm, Hugh C. Cutler,

and Roger Grange, Jr. (528 pp., 179 figs., 4 tables, $8.00. Fieldiana: Anthropology, Volume 40. Chicago Natural History Museum, Chicago, 1952.)

Prior to the excavation of Tularosa and Cordova Caves, available information on Mogollon artifacts, other than those of stone and pottery, was sketchy and only occasionally could they be correlated with the established chronological sequence. However, the excavation of these two caves, located in the heart of the Mogollon country near Pine Lawn Valley in southwestern New Mexico, provided an unparalleled collection of Mogollon perishable materials, assignable to specific chronological phases, and ranging in age from pre-ceramic, with Carbon 14 dates averaging 200 B.C., to late ceramic estimated to date post A.D. 1000

The report, profusely illustrated with excellent photographs and drawings, is a model of descriptive presentation. Following a brief account of the caves and their location, the excavation methods and stratigraphic data are discussed. In both caves the material was removed in blocks 20 cm. thick and 2 meters square. Apparently little attention was paid to the natural stratigraphy; the assignment of each block to a particular Phase was made on the basis of the ceramic assemblage found in that particular block. In Tularosa Cave the majority of the deposits were seemingly of uniform appearance, and this method offered the best technique for seriating the deposits. However, the dangers inherent in this methodology is apparent from Cordova Cave where levels 2 and 4 in square 2RI, since they contained no pottery, were assigned to the pre-ceramic phase. An inspection of the diagram (Figure 25) of the natural stratigraphy shows that a large pit had been cut into these levels from an overlying horizon, which mixed later materials, but no pottery, with the earlier artifacts. Significantly, pottery, including whole vessels, occurs in adjacent blocks at lower levels. The ever-present problem of intrusive pits apparently caused some difficulty for Martin and his associates. Thus, there is a possibility of more than occasional mixture of the materials. For this reason this reviewer is inclined to suspect some of the features, such as cotton (p. 211) and the bow and arrow (p. 341) which are tentatively attributed to the pre-ceramic occupation on the basis of one or two associations. For the most part, however, the two caves provided such an abundance of artifacts that there can be little doubt as to the approximate time that new features made their appearance in this portion of the Mogollon area.

According to the Carbon 14 dates from Tularosa Cave, pottery was being manufactured in the Mogollon area of southwestern New Mexico 500 years before this craft appeared in northern New Mexico among the San Juan Anasazi. This information not only establishes the priority of the Mogollon in the field of ceramics, but also lends considerable support to those who have postulated a southern origin for Mogollon and Anasazi pottery techniques.

Martin and his colleagues have assembled a convincing body of data to support their interpretation of a cultural continuum for the Mogollon from the pre-ceramic through the late ceramic phases. In Tularosa and Cordova Caves the pre-ceramic levels produced a lithic assemblage similar to that from the Cochise-like Wet Legget Site which has Carbon 14 dates at about 2500 B.C., or 2000 years earlier than the dates for the Cave materials. From the caves, however, there is evidence for maize agriculture, suggesting that the Wet Legget peoples may also have been at least part-time farmers. This possibility is further supported by the finds at nearby Bat Cave, where maize was

recovered from deposits which are apparently as old as the Wet Legget materials. In Tularosa and Cordova Caves the Cochise-like lithic materials were found associated with wickerwork sandals, flexible cradles, netted bags, atlatls, darts, coiled basketry, and unfired pottery. A single arrow shaft indicates the possibility of the bow and arrow.

There were few changes in the complex at the beginning of the Pine Lawn Phase, dated at 150 B.C. and marked by the introduction of pottery. A new style of sandal, twisted cord sashes, fur and feather cord blankets, twined basketry, and a few other new and minor features appeared at about that time. But essentially the culture continued with minor modification during this and the following Georgetown Phase. It is interesting to note, however, that the stratigraphic tests do not support the previously held view that the pottery type Alma Rough appeared before Alma Plain in the Pine Lawn Phase, or that Alma Rough was more popular during the early part of this Phase. As a result there is considerable doubt about the seriation of houses at SU site which was based on this apparently incorrect assumption.[1]

At the beginning of the San Francisco Phase, which Martin correlates with Anasazi Pueblo I (ca. A.D. 700), there was a pronounced "cultural shift"—the bow and arrow were either introduced at this time, or became increasingly popular; new sandal types came into use but did not replace the earlier types; several new tools, ritual artifacts, cotton cloth, new weaving techniques, and a new maize type appeared. These, together with the changes in ceramics and architecture which are known from excavation in open sites nearby, indicate that important new influences were active at that time. Probably many of these influences had an Anasazi origin.

Another "cultural shift" is evident at the beginning of the Reserve Phase, when additional Anasazi influences reached the area and led to the introduction of black-on-white pottery and multi-room pueblos, accompanied by a few new weaving techniques and tool types.

One of the most valuable contributions of this report is the detailed distributional studies of perishable materials which were made by Bluhm and Grange. These analyses were aimed at an evaluation of the differences between the Mogollon, Anasazi and Hohokam at various time levels. Martin and his associates are among those who have accepted the Mogollon as a culturally separate group from the Anasazi and the Hohokam. It should be noted, however, that this interpretation has not been embraced by all those who work in the area, although the existence of differences among the three areas cannot be denied. Many regard the Mogollon and Anasazi as regional variants of an overall pattern which differs significantly from the desert-dwelling Hohokam. The reader will do well to evaluate many of the conclusions of this report in the light of other possible interpretations less favorable to the pro-Mogollon viewpoint.

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OTHER

African Sculpture Speaks. Ladislas Segy. (254 pp., 276 illus., $7.50. A. A. Wyn, Inc., New York, 1952.)

[1] Paul S. Martin, John B. Rinaldo, and Ernst Antevs *Cochise and Mogollon Sites, Pine Lawn Valley, Western New Mexico*, Fieldiana: Anthropology, Volume 38, No. 1, p. 196. 1949.

The book deals with the "so-called primitive Negro Art of West Africa that flourished up to a generation ago . . ."

Segy declares that the primary function of this art was an expression of an "inner reality" rather than of a natural perception of nature. The author conceives of "inner reality" as sentiments based on universal human experiences—such as fear. Accordingly, he recommends that the observer rely on intuition to discern the emotional content. He explains the current acceptance of African art as due to the satisfaction of the same needs in our own emotional make-up that prompted its acceptance in West Africa. The emotional experience we respond to in African art should cause us to re-evaluate our relationship to art in general. Contemporary African art is considered inferior to the art analysed by Segy because the animist concepts which prompted them are no longer there, due to European cultural and technological penetration.

Herskovits (*Backgrounds of African Art*) challenged the generalization that all contemporary African art is inferior to art of a "primitive" period since the majority of Africans are not in direct contact with Europeans, nor has their way of life been "entirely disrupted."

The author characterizes the similarity between African art and modern art "as the coincidental meeting of two directions, rather than as a time when Western art fell under African 'influence'." And, he points out the obvious disparity between the intentions of the European artist and the African artist. The European is intent upon the discovery and arrangement of original symbols with a meaning to him personally. The "primitive" African artist worked within a tradition whose forms and meanings are shared by the community-at-large.

This book suffers from a defect that characterizes most works on African art: generalizations drawn from sculpture seen in private and museum collections rather than based on field work. Such generalizations as Segy's, about motivations and purposes of the art, the artist, and the reaction of the community-at-large to the art can only be derived from field work.

Segy's dependence on intuition to understand African art assumes universal aesthetic principles. It presupposes a universal reaction to the same symbolic stimuli regardless of cultural conditioning, in direct contradiction to the principle of cultural relativity.

The book contains a unique guide to market values for collectors. The well-coordinated text, the numerous illustrations and the extensive bibliography in English, French and German make this book a good starting point for the student. However, a student of anthropology cannot accept the conclusions of the author.

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The Salvage. DOROTHY SWAINE THOMAS with the assistance of CHARLES KIKUCHI and JAMES SAKODA. (xii, 637 pp., 26 charts, 36 figs., $7.50. University of California Press, Berkeley and Los Angeles, 1952.)

This is the second of the two studies by Dorothy Swaine Thomas and associates on the social aspects of wartime evacuation, detention, segregation, and resettlement of the Japanese-American minority. *The Spoilage,* which appeared in 1946, "was a

case history of that group of persons evacuated from the West Coast in 1942 whose status was impaired: those of the immigrant group who returned, after the war, to defeated Japan; and those of the second generation who relinquished American citizenship." One out of six evacuees fell into this category. *The Salvage* calls our attention to the three out of six evacuees who chose to relocate themselves in the East and Middle-West during 1943 and 1944.

This book is divided into two parts. In Part I the authors, using statistical data from several sources, and primary data gathered by the field workers in the Evacuation and Resettlement Study group, trace historically the main political and administrative actions that impinged upon the Japanese immigrant group and their children. The authors define group trends and intergroup differentials in demographic, economic, and cultural terms. Fragmentary data bearing on social organization and political orientation are then synthesized. The section on forced migration includes a summary of *The Spoilage* as well as statistical analysis of the differential incidence of segregation. In the final section of Part I the authors present an analysis of the factors bearing upon the three courses taken by the evacuees—*spoilage*, *salvage*, and *residue*. They point out that while the forced mass migration from the West Coast was unselective, the course taken while in the camps was channelled by social factors. The *spoilage* were people predominantly from "prejudiced" California, engaged in farming activities, were "Japan-polarized" Buddhists in religion, and moved from other centers to Tule Lake, the detention center for those who renounced American citizenship, in family units. On the other hand, the *salvage* were represented by evacuees predominantly from the more "tolerent" Pacific Northwest, engaged in urban pursuits, Christians or secularists, and migrated from the camps as individuals not in family groups. Furthermore, seven out of ten of this group were between the ages of 15 and 35. The *residue* represented the core of the evacuee group that in 1942 had been physically, emotionally, and demographically incapable of finding jobs and homes in other areas.

Part II begins with a summary statement of the "mass phenomena" described and analyzed in the preceding 128 pages, and provides context for the fifteen life stories that constitute the bulk of the book. These represent "the train of individual experiences as observed and reconstructed by resettlers in the Chicago area" and give depth to the study. Between April, 1943 and August, 1945, Kikuchi collected sixty "complete" life histories. A detailed outline used to organize the data, criteria for selection of informants, and description of the interviewing method follow. The life histories of the eleven males and four females presented in this volume were selected because they were thought to be the best complement to the treatment of mass phenomena. The basis for selection, however, was "influenced most directly by sections on agricultural adjustment, urban enterprise, and occupational mobility, with particular reference to Nisei."

The book is well organized and highly readable. Pertinent statistical data are analyzed and collated to excellent advantage. And, if at times the graphic presentations tend to compress individuals into seemingly rigid categories, the life stories skillfully recorded bring us back to the wide range of individual experiences. Part II ends abruptly with the presentation of the final life history. Perhaps an epilogue is needed.

The appearance of the book eight years after completion of the field work makes the subject-matter seem a bit dated. One feels the need for another volume which would

present the long-range study of the *salvage*, as well as of the *residue*. This does not, however, diminish its value as a significant contribution to the field of minority relations or to applied social scientists. The book should be of definite interest to those who contemplate the study of relocation now in progress, or anticipated, on the various Indian Reservations, and in countries where large-scale development of natural resources call for selective migration of sub-cultural groups.

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Shona Customary Law: With reference to Kinship, Marriage, the Family and the Estate. J. F. HOLLEMAN. (401 pp., 6 tables, 22 diagrams, $9.00. Oxford University Press, New York, 1952.)

This is another excellent contribution to the study of kinship and marriage in Negro Africa. One of the few criticisms of the work, and one that is often leveled at the works of contemporary British or British-trained anthropologists, is that it confines itself perhaps too rigidly to social structure at the expense of other aspects of culture. It is an exhaustive treatment of the sociopolitical system of Mashonaland in Southern Rhodesia, and the research on which it is based was carried out in three native reserves. As the title indicates, the work deals with "some aspects of Shona customary law—kinship law, marriage and family law, estate law." Holleman does not include customary law as it pertains to cases on assault or homicide, for example.

Holleman describes the social organization of the Shona, beginning with the village (which is often an extended family) and expanding it to include the tribal ward and the tribe. The major portion of the work, however, deals with customary law relative to choosing a mate, marriage preliminaries, the marriage contract and bride-wealth (*rovoro*), parental rights and responsibilities, divorce and inheritance. Several diagrams and tables are utilized to portray the kinship structure, and extensive use is made of case studies and footnotes for illustrative and explanatory statements.

One of the most salient features of the work is the emphasis on the flexibility of customary law. Although Holleman has abstracted the precedents and guiding principles, he stresses that "a satisfactory solution of the conflict between the parties often matters more than a correct interpretation of the legal principles involved." It is highly possible that this flexibility is characteristic of other groups of the area, if not of peoples south of the Sahara whose culture exhibits a similar degree of complexity. Thus customary law, rather than being dictative is accommodative; rather than prescribing the findings of a chief or council, it suggests a course of action.

In addition to an outline of traditional law, the changes that have occurred under British administration are also included, though the delineation between customary law and laws introduced by governmental directive since annexation is not always clear. This occasional lack of clarity, however, may be attributed to acculturation and the memory of informants rather than the field worker. Included in the appendix is a critique of recent marriage regulations imposed by the government that to Holleman represent an unrealistic approach to the problem and a lack of appreciation of Shona culture.

One of the shortcomings of the work is a complete lack of discussion of the supernatural. While reference is made in passing to sacrifices and witchcraft, there is no account of the religious concepts of the Shona, or the role the supernatural plays in daily life. Though the work is admittedly confined to the sociopolitical aspects of the culture, at least a brief outline of their religion would have given a more comprehensive picture of Shona social structure. Though human agencies may play the primary role in enforcing custom or the decisions of those in authority, one suspects that fear of retaliation by the supernatural is a factor in forcing compliance with customary law pertaining to those aspects of Shona life under discussion. Since this work represents only part of the investigation of the people of Mashonaland that Holleman has completed or propoposes, it is to be hoped that other components of Shona culture not described in this work will be forthcoming.

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Le Chamanisme et les Techniques Archaïques de l'Extase. MIRCEA ELIADE. (447 pp., 1300 fr., Payot, Paris, 1951.)

This is a most erudite work. While the author's stated intention was not to write an exhaustive study of shamanism, both content and numerous bibliographic references attest to extensive research. The work is directed primarily toward the cultivated layman, not the specialist, but since the author has drawn heavily on ethnographic literature, it seems proper in this review to consider the study from the point of view of the professional anthropologist.

In characterizing shamanism, Dr. Eliade points out with reason that the modern tendency to equate the term shaman with such general terms as medicine-man militates against an understanding of the phenomenon of shamanism (p. 17). The characteristics which he suggests will provoke discussion, but this is in itself valuable. The characteristics are: technique of ecstasy, specifically a trance state in which the shaman's soul leaves the body to travel to heaven or hell; possession by a spirit, over which the shaman remains master; magic flight and mastery of fire; apartness of shamans from their society; presence of a High God cult in association with, but not closely integrated into shamanism.

Taking Central Asia and Siberia as the area where all these characteristics of shamanism are found most closely associated, Dr. Eliade devotes eight chapters to a description and analysis of shamanistic practices there. In four chapters he considers shamanism in the Americas, Southeast Asia (Malay Peninsula) and Oceania, among the Indo-Europeans (from Scandinavia to ancient India), and in Tibet and China. These four chapters were, to the reviewer, the most rewarding in the volume.

The theoretical scheme within which the rich data are presented is less appealing to the American anthropologist. Dr. Eliade describes himself as an historian of religion rather than ethnologist. As religious historian he presents the thesis that: the dialectic of the sacred tends to repeat ad infinitum a series of archetypes through time and space; no form is exempt from degradation and decomposition; the concept of spontaneous reversibility of any religious position is important. For this reason he does not allow himself to be too much influenced by the results of ethnological cultural history;

the divers types of civilization are, indeed, organically tied to certain religious forms, but this in no way excludes spontaneity and, in the last analysis, the anhistoricity of the religious life. He claims that the essential characteristics of shamanism are archaic, to be found as isolated elements in all parts of the world, although they happen to be most thickly clustered in Central Asia and Siberia. The concepts of reversibility and anhistoricity of religious life reinforce his insistence on the concept of a primeval High God, and appear to explain, for example, why China, with a much longer history of civilization than Europe, should have "almost all the constitutional elements of shamanism" (p. 401), while early Europe had very few.

Despite the theoretical bias, which is most pronounced in the introduction and conclusions, this work has much of value for the American culture historian. It not only brings together a great deal of data on shamanism, but offers some very stimulating analyses along the way.

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Sexual Behavior in Western Arnhem Land. RONALD M. BERNDT and CATHERINE H. BERNDT. (247 pp., Wenner-Gren Publications in Anthropology, No. 16, $4.00, New York, 1951.)

This description of certain aspects of sexuality in the Australian aborigines may prove exceedingly valuable to ethnologists, but to a non-anthropologist who is interested in sex research it is subject to two serious criticisms. First, the interpretation of various kinds of evidence is naive and uncritical. Second, the choice of data to be collected was subject to an unfortunate bias.

For evidence concerning sexual practices and attitudes the Berndt's relied chiefly upon three sources. One category involves supposedly factual accounts related by native informants about their own sexual experiences or those of other individuals. It is not always possible to determine whether the descriptions are first-hand or merely stories that have been told to the informant by someone else. Likewise when the account refers to past events there is no way to allow for forgetting or distortion, and some of the incidents are admittedly historical.

A second source of evidence includes the tribal myths as well as certain traditional religious and ceremonial activities. The practice of story-telling is highly developed and story-men relate the popular myths at public gatherings. Many of the myths and several important annual ceremonies have to do with sexual life, although it may be treated in abstract, symbolic fashion. The third type of evidence is found in the "gossip songs" which are composed and sung by song-men. These stylized compositions tell about various social incidents which occur in the community life. Very often the songs are about marital behavior and relations between lovers.

The general picture of sexual behavior presented in the book under discussion is a compound of evidence from all of these sources. Evidently the content of tribal myths is considered a valid source of information concerning every-day behavior, while the veracity and accuracy of stories recalled by informants is not questioned. The usual method of presentation is to make a statement or generalization about behavior and then to illustrate the point by the direct quotation of some conversation or some mytho-

logical story. The reader has no reliable way of separating the authors' interpretation from their factual data since there is no means of establishing the representativeness of the selected case histories. This method of presenting evidence gives no opportunity for quantitative treatment of the data but it certainly overwhelms the reader with a great quantity of quotations the value of which can legitimately be questioned.

It is unfair to criticise an author for failing to achieve a goal which he was not trying to achieve, but it must be noted that the title of the Berndts' monograph is misleading. They present the reader, not with any objective and systematic analysis of sexual behavior, but rather with a loosely organized accumulation of songs, myths and stories dealing with certain sexual mores. Emphasis is laid upon the cultural rules for heterosexual coitus and the social consequences of obeying or violating these rules.

The authors state specifically that they are not concerned with the physical act of intercourse, nor with foreplay, nor with the physical and emotional results of erotic activity. These are regarded as "incidental and personal" and lacking in general social importance. Sex play in childhood receives little attention, being covered in five short paragraphs. The entire subject of sexual abnormality is dealt with in a page and one-half.

It becomes clear from reading this volume that the people with whom it deals are far from inhibited in their sexual lives. There seem to have been many opportunities for the authors to obtain fairly direct information about native sexual behavior. If this assumption is correct it is to be hoped that the Berndts or other investigators will extend their studies to provide science with a more intensive and fundamental analysis of *Sexual Behavior in Western Arnhem Land.*

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BOOK NOTES

Ethnobotany of the Ramah Navaho. Paul A. Vestal. (ix, 94 pp., Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University, vol. XL, no. 4, Peabody Museum, Cambridge, 1952.)

This publication is one aspect of a considerable amount of work that has been done on the Ramah Navaho within recent years by a number of investigators. Happily, Vestal brings together not only the results of his two seasons of field work but also the ethnobotanical data bearing on the Ramah Navaho as gleaned from the notes of Kluckhohn, Bailey, Tschopik, and Wyman.

This study presents the results of investigation on the utilization of native plants for such purposes as food, medicine, dyes, ceremonial use, smoking, string and rope, basketry, toilet accessories, household articles, fuel, and arrow poison. Also, there is a treatment of cultivated plants in which the author does a creditable job not only in presenting a statement of the growing and utilization of cultivated plants but, equally important, a brief statement of techniques of cultivation. Moreover, he has made a successful attempt at giving a judgment of the extent of cultivation in terms of specific crops in relation to acreages as well as of the relative importance of specific native food plants. Of considerable interest is the fact that Vestal's study of the nature of the Ramah Navaho concept of plant classification confirms the general conclusion reached earlier by Wyman and Harris with regard to Navaho plant nomenclature in general. The list, as well as the statement covering Navaho names for plants, is most useful, as are the special statements of the several categories of plant utilization.

Interesting and very useful features of the publication are the Table of Uses, the Botanical Index, and the Common Name Index which make it easy for the reader to make the maximum use of the study.

The field studies on which the paper is largely based have been admirably executed, and the results of the investigation have been presented in an exceptionally well-organized manner. From this reviewer's point of view the only important fault of the study lies in the fact that it is very largely botanical in outlook and fails to investigate the interrelationships between the Ramah Navaho and their plants, in other words the cultural aspects of plant utilization. (EDWARD S. CASTETTER)

Die Blattspitzen des Paläolithikums Europa. Gisela Freund. (249 pp., 16 figs., 1 map, Ludwig Rohrscheid Verlag. Quartär-Bibliothek, Bd. I, Bonn, 1952.)

In addition to the Solutrean of Western Europe and the appearance of leaf-shaped points in the Late Acheulian and Mousterian complexes, Dr. Freund discusses in considerable detail the so-called "Pre-Solutrean" of Central and Eastern Europe. According to the author, the latter neither represents the forerunner of the West European Solutrean, nor is it supposed to give rise to a true Solutrean development in Central and Eastern Europe. Indeed, why she insists on retaining this exceedingly unhappy term is something of a mystery. Nevertheless there is no doubt about the fact that Dr. Freund's penetrating analysis of the problem brings into clearer focus the relevant data pertaining to the Middle and Upper Palaeolithic industries characterized by the presence of leaf-shaped points. Certainly anyone who has ever attempted to synthesize such an enormous mass of published materials and museum collections will appreciate the magnitude of the research project she has undertaken. However, it is to be hoped that the author can be induced to bring out a condensed version of the very important subject-matter treated in her very fine book. Notwithstanding the fact that the question of the leaf-shaped point industries is one of the biggest puzzles in Palaeolithic archeology, only a very few specialists will be prepared to read through some 325 pages of text, a good deal of which is concerned with detailed stratigraphic considerations, in order to gain an understanding of Dr. Freund's views on this fundamental problem. (HALLAM L. MOVIUS, JR.)

Die Heilige Hinterecke im Hauskult der Völker Nordosteuropas und Nordasiens. Gustav Ränk. (245 pp., 60 plates, Folklore Fellows Communications no. 137, Academia Scientiarum Fennica, Helsinki, 1949.)

Ränk's monograph represents a comprehensive collation on a recurrent feature of north Eurasian religious beliefs, the sanctity attached to the rear corner of a dwelling. The author links this sanctity to a patrilineally-inherited male cult for ensuring food supplies. He further concludes that the ritual disabilities of women derive from the potential conflict of their inherited spirits with those of the men. A special feature of the rear corner, a ritual entrance-way for game and for spirits, may, he infers, be an ancient loan from Near Eastern temple design.

In general, I would regard this study as one of uneven value, most significant as a source for abundant ethnographic detail. Its summaries of the household cults of cultural groupings within the vast expanse of northern Eurasia, such as those of the northeast European peasants and the Tungusic peoples, are excellent. In contrast, the overall analysis appears weak and forced. Thus, while some contacts and syncretism between northeast European peasant and Uralic clan-cult beliefs are evident, the historical ties and underlying rationale of the former appear to me to affiliate much more with agricultural house cults, such as those known in the Danubian Basin since Neolithic times. Even in the north Asiatic forests, the sanctity of the house posts and the cult of the World Tree must be divorced historically and functionally from the rear-corner complex. Finally, awareness of the tremendous world distribution of such concepts as the ritual impurity of women or the ceremonial rear entrance would introduce greater caution in historical reconstruction than Ränk has shown. (D. B. SHIMKIN)

A Pacific Bibliography: Printed Matter Relating to the Native Peoples of Polynesia, Melanesia, and Micronesia. C. R. H. TAYLOR (xxix, 492 pp., £2/2/-. Polynesian Society Memoir, Volume 24, Wellington, New Zealand, 1951).

Taylor's excellent bibliography is divided into four major sections: Oceania in general, Polynesia, Melanesia, and Micronesia. Each of the three latter regions is in turn divided into its component island groups. Under these areal headings, the approximately 10,000 bibliographic references are classified according to conventional ethnographic topics. A useful feature of the book is the careful inclusion of previously published bibliographies. Concise instructions for the use of the bibliography and a clear explanation of its organization are part of the introductory matter. Although of primary interest to the anthropologist, the book is designed for a broad scholarly public and is an indispensable tool for all students of the Pacific islands. (ALEXANDER SPOEHR)

The Bechuanaland Protectorate. A. Sillery. (xii, 236 pp., $6.50. Oxford University Press, New York, 1952.)

The author of this volume was Resident Commissioner of the Bechuanaland Protectorate from 1946 to 1950. He begins by setting forth the limitations of his study: it is no more than an "outline of events," which makes little reference to "the customs and characteristics of the people of the Protectorate." Sillery "made little attempt at original research" (p. vii), and refers his readers to the works of Dr. I. Schapera for materials on the people of Bechuanaland.

Even within these limits, the work leaves much to be desired. Sillery does not succeed in giving any insight either into the nature of the peoples of Bechuanaland with whom the Europeans came in contact in the early Nineteenth Century, or into the historical forces (economic, religious, etc.) that "pushed" the British into the area. He does stress the important fact that the Protectorate, as a form of colonial administration, grew out of the special need arising from the conflict of the Boer and the Briton in South Africa. His discussion of the peoples of Bechuanaland is that of a humane but paternal and ethnocentric administrator, rather than one exhibiting an ability to analyze the behavior of these peoples, either at the time of initial contact, or today. And, unfortunately, he devotes only a small section of his study to his own specialty, the problem of colonial administration. There is a useful section on "Traditions and History of the Tribes," though it is only a history in a limited political sense, dealing mainly with the lines of succession of the chiefs. In the face of real need for books which assemble scattered data in the field of African studies, it is unfortunate that Sillery's effort lacks desirable breadth and analytic ability. (DAVID W. AMES)

The Pattern of a Dependent Economy; A Study of the National Income of British Honduras. N. S. Carey Jones. (xvi, 162 pp., 13 tables, $4.00. Cambridge University Press, London, 1952.)

The author's purpose is to present a national income study of a dependent economy in a way valuable for the representatives who formulate and administer government policy, and that the public may understand such policy. The general description which accompanies and explains the tables of figures provides numerous insights into the social effects of economic policy and economic change. Unfortunately, it also provides one or two startlingly naive cultural stereotypes. The author has considerable personal knowledge of the Gold Coast and Northern Rhodesia. This enables him to select some of the economic problems, and patterns of economic and social change in British Honduras, that may be general to dependent economies. There is little discussion of the local scene as such, and the book is not written for the anthropologist. Nevertheless, the last three chapters contain some provocative ideas on such topics as the problems involved in raising the standard of living for the indigenous population; and how the local people are affected by foreign investment in the colony, and by fluctuations in world markets. (STEPHEN A. RICHARDSON)

Brief Communications

A Mechanistic Theory of Human Evolution

Modern biological studies indicate very strongly that natural selection acting upon random genetic changes is the basis of organic evolution. In general, this idea has penetrated into published studies of the evolution of man only very recently. Even many recent works have either a teleologic bias or are simply descriptive without attempting explanations. Mechanistic theories of human evolution are poorly represented.

Washburn (1950) gives an interpretation of primate evolution up through the assumption of the upright posture in the human line on the basis of adaptational selection theory and Simpson's (1944: 197–217) evolutionary modes. However, he gives no explanation of the later stages of human evolution, summarized by Weidenreich (1947) as a great expansion in the size of the brain and a reduction in the size of the face, observable during the course of the Pleistocene. A mechanistic adaptation theory of this, the essentially human part of the evolution of our species, is readily available but is not a part of general anthropological thought. This is revealed by an examination of a number of recent texts in physical anthropology and general textbooks which include accounts of human evolution; nor do recent symposium volumes (Cold Spring Harbor Symposium 1950; Kroeber 1953) show a general awareness of the probable adaptive nature of this human evolution. It therefore seems probable that this theory is not generally taught in the universities.

Exceptions to this general rule are Dobzhansky and Ashley Montagu (1947), who have given an interpretation in several respects similar to the basic theory of this paper. Ashley Montagu incorporates these ideas into his recent textbook (1951: 366–372), and Dobzhansky (1950: 399–400) emphasizes these points in a symposium volume. However, the extent to which the general theory fits in with the fossil record, the archeological discoveries in the Old World, population genetics and modern paleontological theory to form a consistent picture has never been emphasized adequately, even in the advanced discussions at the recent international symposium (Tax et al., 1953: 28–31; 259–272).

Weidenreich (1947: 1) cites as evidence for an orthogenetic interpretation of man's evolution in the Pleistocene the fact that fossil macaques contemporary with Peking man and in the same habitat are almost identical with present day forms, while the human line has evolved greatly. The presence of the two forms in the same territory does not mean that they are ecologically similar, however. It seems certain that the two groups were and are in different adaptive zones, to use Simpson's (1944: 188–191) terminology.

The human adaptive zone is readily abstracted in spite of the enormous variety of ways man adjusts to various environments. Man is primarily a cultural animal, adapted to learned behavior through the principal medium of symbolic communication. The large and highly organized brain gives the necessary ability to carry on this way of life and to deal with a great range of situations beyond the comprehension of other animals.

Carpenter's studies (1934, 1940) show that other higher primates are to a considerable degree pre-adapted for such a way of life, being gregarious and having varied

vocal behavior which is to some degree meaningful to other members of the group. The brains are relatively large and complex. Ancestors of the human group must have made the shift over to symbolic communication to initiate specifically human evolution. Such an adaptive change corresponds to Simpson's evolutionary mode, the *quantum evolution*. Once such a shift toward this new adaptive zone was initiated, a high selective advantage for individuals better adapted to learned behavior and symbolic communication must have ensued. This would give a non-teleological explanation for the very rapid expansion of the brain during the Pleistocene evolution. The fossil record indicates that this phase of human evolution fits Simpson's model of a *phyletic evolution* very closely, a progressively improving adjustment to an adaptive zone. The progressive increase in brain size is interpreted here as indicating the process involved in the evolution of a species adapting better to a cultural way of life. In terms of selection theory, individuals with better brains would be better adapted to a cultural environment, with resulting superior viability and greater reproduction of culture-adapted characteristics.

The decrease in face size noted by Weidenreich may not be explainable in exactly the same terms. It may be that the large dentition and faces of early hominids and their probable man-ape ancestors were strongly selected for, and that the decrease in size of face and teeth during the Pleistocene represents a relaxation of previous selection. It may be noted that the great apes and man-apes are all prognathous, but many monkeys have short faces.

Wright (1949a: 387) observes that the development of basic adaptive advantages in a group usually results in an adaptive radiation, in which descendant groups enter many new adaptive zones. There is then a rapid new development of species, genera and higher categories. This has not been the case with man. In spite of his enormous biological success, only one genus and species of the hominids exists today.

Two factors have probably contributed. First, the amount of time was quite short for any great differentiation. More important, however, is the probable maintenance of continuous gene exchange through interbreeding, made possible by the continuous distribution of the species resulting from its wide geographic adaptability. The common cultural adaptive zone prevented other genetic isolating processes from taking place. If this was the case, it seems likely that Weidenreich's (1949: 156) insistence that there was only a single species of man throughout the Pleistocene is correct.

The rapid evolution during the Pleistocene was probably facilitated by the nature of the population structure. Wright (1949a, 1949b) has shown how a widespread species divided into numerous fairly sparse local populations with partial reproductive isolation is in a better position for continuing evolution in the absence of changes in the environment, and has a greater capacity to respond rapidly to such changes. This population distribution allows greater genetic variability to be developed, maintained and exploited in the evolutionary process. Advantageous genes and their combinations in local populations lead to their spread throughout the species through migrations and outbreeding, to be followed once more by local partial isolations. Such an arrangement is not dependent upon new mutations for rapid selection and evolution.

The condition of contemporary non-agricultural peoples indicates that human groups were probably small in population and partially isolated reproductively by geographical and cultural factors during much of the Pleistocene. The archeological and

fossil record reveals that men spread throughout much of the Old World during that time. Howell (1951, 1952) has even given evidence of partial isolation for one group, the European Neanderthals, and indicates their subsequent replacement by other peoples. The human population situation thus probably approximated the ideal one depicted by Wright.

The South African man-apes are now regarded by many anthropologists as representing the ancestral human form, and may actually have included direct ancestors. To date no artifacts have been definitely associated with them, and certainly no tool-making traditions, in spite of their presumptively relatively recent date. Definite tool-making traditions seem to begin during or just before the Pleistocene. This is here assumed to be evidence for a cultural way of life. Thus the approximate contemporaneity of beginning expansion of the hominid braincase and the first good evidences of culture tend to support the theory outlined above, based on the probability that human evolution can be explained mechanistically as an increasingly adequate adaptation to a cultural environment.

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Notes on Two Dakota "Holy Dance" Medicines and Their Uses

A once powerful ceremonial organization of the Dakota which is little known at present is the *wakán-wacípi*, or "holy dance." This society corresponds to the *Midéwiwin* lodge of the Ojibwa, Cree, and Central-Algonkin tribes and the similar medicine lodges of the Southern-Siouan groups. The society was largely made up of shamans skilled in sleight of hand, and a main feature of their ceremonies was the magical "shooting" of small objects into the bodies of members by other initiates and their subsequent removal by "magic" means. Members of the organization were also renowned for their knowledge of herb medicines.

On November 14, 1952, the author was privileged to acquire two herb medicines connected with the holy dance society and to be taught their uses by Mrs. Eva Littlechief, a Yanktonai Dakota woman living in Bismarck, North Dakota. Mrs. Little-chief is the daughter of the late chief Red-fish, who was a noted shaman and a prominent member of the holy dance society at Cannonball, North Dakota. Red-fish's medicine bundle has been described by the author in a previous paper (Howard 1952). Mrs. Little-chief was taught the uses of the two medicines by her father shortly before his death in 1929.

Red-fish was skilled in both the use of herb medicines and in the shamanistic illusions used by the members of the holy dance to impress the laity of the tribe with their "power." His most famous trick was "making a plum tree grow." Red-fish would begin the act by bringing a plum branch into the medicine lodge and sticking it into the dirt floor. He then began to dance around the branch. As he danced the branch produced blossoms, leaves, and finally plums. As proof that these plums were genuine Red-fish would pick them and distribute them among the spectators. He was well known for this trick, not only among his own people, the Dakota, but among the Omaha and Winnebago of Nebraska as well.

The author first learned of Red-fish's herb medicines while driving Mr. and Mrs. Little-chief to a grass dance. On this trip Mrs. Little-chief talked of her father and his herb medicines and mentioned that she had been taught a few of them. A few days later the author called on her at her home, presented her with a ham sandwich and a package of cigarettes (in lieu of the dog feast and tobacco formerly accompanying any request for information from a shaman) and asked for more information on Red-fish's medicines.

Mrs. Little-chief produced a large cloth bundle in which two different bunches of dried roots were tied. She gave the author a specimen of each type, together with a large jar of raccoon fat, saying "I can't show you how to grow a plum tree (referring to her father's magical performance) but I can give you some medicines."

The first herb she called *sí"kpe ta-wóte*. (lit. muskrat his food). It has been identified as sweet flag (*acorus calamus*. The identification was made by Dr. O. A. Stevens botanist at North Dakota Agricultural College, Fargo, North Dakota.) Mrs. Little-chief stated that this root, when chewed regularly by a person afflicted with diabetes, would cure the disease within a few months. She cited examples of Indians suffering from diabetes, who had been "given up" by white doctors, but were later cured with this herb.

The second plant she called *pó-ipíye* (swelling-it heals). Its botanical identity has

not yet been established. The raccoon fat is used in connection with this medicine. The herb, as the name implies, is used for swellings, and also for cuts and simple fractures A small amount of the root is ground up and sprinkled upon the cut or swelling. Raccoon fat is then painted around the wound. Mrs. Little-chief stated that grease from any wild animal would serve as well, though grease from a tame animal would be ineffectual.

In the case of a fracture the same procedure is followed as with a cut or swelling, but heat application is used as well. The Indian doctor first gently pulls the limb and sets the bone. He then applies the herb and raccoon grease. Next he warms his hands at the stove or fire and places them on the broken limb at the point of fracture. This warming is repeated for several hours at a time, and is said to give the patient relief from pain. Splints of matted bison hair and rawhide are known to have been used for fractures by the Dakota (Densmore 1918: 261), but were not mentioned in connection with this treatment by Mrs. Little-chief.

The above-described remedies are still in common use. Mrs. Little-chief stated that in the past year several Indians, not being able to afford the services of white physicians have come to her to be treated. It is probable that many other herb simples and medical practices formerly connected with the holy dance society and other Dakota shamanistic cults, such as the buffalo and bear doctors, have continued in a similar fashion, being administered by anyone who has bothered to learn their uses.

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TIME PERSPECTIVE IN PLAINS INDIAN BEADED ART

The following is a brief description of the initial effort at The Science Museum of St. Paul, Minnesota, to establish on the basis of data (elements of design, techniques of construction, basic and accessory materials employed, bead types and colors) from known specimens in American museums a method for determining the relative age and probable tribal origin of unknown bead-decorated objects from the North Central Plains area.[1] Such a method would, if scientifically acceptable, prove a boon to the small as well as to the large museum, for an extraordinary large proportion of Plains Indian bead-decorated objects must be regarded as unknowns.

The study was conceived originally because of the difficulties and uncertainties arising from an attempt to classify and catalog a new collection of Plains Indian beaded objects about which little was known, either of the provenance of the specimens or their dates of collection. The usual procedures were followed. Kroeber, Wissler, Lowie, and other authorities were studied, the Bureau of American Ethnology Reports and the Anthropological Papers of the American Museum of Natural History searched for possible clues to the origin of the specimens. The objects were measured against available descriptions of construction patterns, Kroeber's and Wissler's design elements, the

variously mentioned tribal peculiarities, and even chance photographs. Eventually, a good number of the pieces in the collection were duly but tentatively identified.

In the course of the cataloging, we attempted to set down in some kind of ordered series those criteria of identification used in the past by the different ethnographers in their descriptive and comparative studies of the various Plains tribes. These criteria we supplemented by data from other and later ethnologists. Based on the literature, we thus built up for most of the tribes of this area a fairly complete and detailed series of criteria useful in identification. Additionally, we compared and coded beads, and we attempted to test other characteristics—fringes, feathers, tinklers, kinds of cloth and of skin—anything definite enough to be comparable.

Most beaded objects may be regarded as a complex of separable traits, traits which are determinable and comparable with similar traits on other beaded objects. It seems probable, due to such factors as tribal preferences or habits, the changing supply of trade goods available at different time periods, and contact, both tribe with tribe and Indians with whites, that the total complex of traits of any object has time aspects, which will reveal within the limits of probability, the approximate age and probable tribal origin of a beaded piece.

Initially our hypotheses were tested distributionally on our own museum collections. Samples from three tribes were selected for comparison—25 Blackfoot specimens, 33 Dakota, and 14 Ojibwa. For a test run we compared only the beads, the percentages of each tribal sample which showed particular beads. The results indicated apparent significant tribal differences, for the frequencies of occurrence of some beads were such that we had to assume some degree of tribal choice in beads—or possibly rather a choice in beads from those made available (due to different bead sources, traders, etc.).

The probable importance of time perspective had been previously suggested during our preliminary listing of useful criteria of identification, and the trade factor implied that time would necessarily have to be considered in any further study. This emphasis on time is, of course, neither very new nor very startling. Nearly two decades ago Strong (1936) stated that the generally accepted characteristics of the cultures of the Plains area were based entirely on distributional studies and that these characteristics are in fact only those of relatively late-in-the-area cultures. More recently Fenton (1952) has called attention to the possibilities of still getting at time perspective in areas such as the Plains. However, it was on the basis of our preliminary studies, that we formulated the following working hypotheses about the use of beads by Plains tribes:

1) that particular types and colors of beads may be specific to or most popular among certain tribal groups,
2) that these bead preferences may, just as types of basic materials, accessory decorative devices, etc. be restricted to, or occur most frequently in, certain time periods—due perhaps to the sheer availability of the beads,
3) that while a single bead type might be by itself insufficient to localize or date beaded objects, tables showing the distribution, frequency of occurrence, and the chronology of all known beads might be useful aids in the identification and dating of unknown specimens. While the tendency certainly exists to use beads over and over again—with new beads appearing only as the old beaded objects are lost, destroyed, sold or given away, relative dating would be calculable by the appearance of new beads.

During six months in 1952, visits were made to eight museums[2] throughout the country for the purpose of examining and recording data from known beaded specimens of the Plains area. By known specimens are meant those dated and localized according to museum accession records. 393 specimens from nine northern Plains tribes and the Woodland Ojibwa were selected. The sample is distributed in time and by tribes as follows:

	Pre-1875	1875	1885	1895	1905	1915	Post 1925
Crow		1	2	34	13	1	
Assiniboin	2		4	30	8	3	
E. Dakota	2	2	6	2	4		
W. Dakota	8	25	17	18	12		
Cheyenne	3		7	31	3	1	
Arapaho	1		3	16			
Blackfoot	1	1	20	33	13	6	1
Gros Ventre			1	15	3		1
Plains Cree		6		1	6		1
Ojibwa			13	3	9		

Because of the obvious difficulties and limitations in working with many museum collections, collections where many specimens are neither dated nor localized, or frequently incorrectly identified, we (1) selected whenever possible only specimens collected in specific areas by competent collectors, and (2) emphasize that all dating is necessarily relative and approximate. While the actual date of accession or collection has been recorded, a specimen collected or accessioned during the period 1876–1885 is considered in our graphic analysis as approximately 1875, as this reflects our judgment that most beaded specimens collected by competent collectors were manufactured during the decade immediately prior to the collection date. Occasional pieces that do not fit this pattern will not, we feel, defeat the overall usefulness of our analysis, and in any event, the method does give absolute terminal dating.

While initially our hypotheses were concerned with the distribution in time and space of bead types, it was never the intention that unknown specimens might be dated and localized solely on the basis of bead criteria, and in recording data from museum specimens, the following kinds of data were recorded:

1) basic materials employed and accessory materials (feathers, weasel skin, tinklers, etc.),
2) design and design elements (using chiefly Kroeber, but also Wissler),
3) stitching techniques and modes of construction,
4) beads by type and color

For each specimen all definite traits within each of the above categories were entered by code number on punch cards.

The method used in analysis of the 393 specimens is essentially archeological. For each of the ten tribes we have indicated on charts and in ascending order beginning with the earliest dated specimen, the presence and absence of each trait in the above categories (Powell 1953). The charts were, for greater facility in representation, converted to graphs and tables to show the relative tribal distribution in time and space of each trait. It should be noted that the periods for which there are valid samples (twelve or more specimens for a ten year period) vary greatly; the Dakota are ade-

quately represented for four decades between 1870–1910; the Blackfoot for the three decades 1880–1910; the Crow for but the twenty-year period between 1890–1910. For the other tribes reliable samples are lacking for longer than a ten-year period. It is hoped, however, that in the near future, the many gaps may be filled in and the chronological areas lengthened for all tribes.

Inadequate though the samples may be, they do permit some comparison of the distributional and chronological spread of the different traits among the ten tribes represented. An example might be the small brassy or silver-faceted metallic beads which are known to have appeared in the Plains in the late Nineteenth Century. Considered in flat perspective, the "center of intensity" would be among the Dakota, followed by the Crow, Arapaho, and Cheyenne. In temporal perspective, metallic beads appear in the Eighties among the Dakota with some use by the Arapaho; they are abundant in the Nineties among the Dakota and infrequent among the Arapaho, Cheyenne and Crow. The period of greatest Crow use of the beads appears to be around and after 1900. Metallic beads of this kind do not show up on Blackfoot, Assiniboin, or Gros Ventre pieces.

A similar use in time is indicated for small metal tinklers. Little mention is found in the literature on these cone-shaped ornamental devices. They appear on most kinds of garments, pipe-bags, and pouches. Lowie (1921) mentioned their use by the Crow, and there are examples in plate illustrations for the Gros Ventre (Kroeber) and Dakota (Wissler). These tinklers appear on our oldest Dakota specimens (1856 and 1860), are frequent in the Seventies, and over 40% of Dakota specimens of the Eighties show the presence of tinklers. Their use in the Nineties declines to about 30% and to about 15% in the decade 1900–1910. Tinklers appear on scattered pieces from other Plains tribes with the Crow showing a 25 to 30% frequency in the 1900–1910 period—double that of the Dakota for the same period. Again, as with the metallic beads, there is no indication that these small tinklers were commonly used by the Blackfeet, Assiniboin, and other northern tribes.

A similar differential use of beads in time and space is indicated, and many other traits show both chronological and distributional variations of significance. For many of the traits, negative evidence would seem to be a valid criterion for the localization of specimens. This is suggested above by the apparent lack of use of metallic beads and tinklers by the Blackfeet and Assiniboin. Beads may turn out to be especially good time indicators, and it may be possible eventually to tie in dated museum specimen beads with the early trade beads being turned up archeologically in the Plains area. Here, however, we wish only to suggest the possibilities of the method and the techniques. They are, we feel, of both practical and theoretical significance. Practically, with more adequate samples, one should be able to fit unidentified beaded specimens from this area to charts and graphs and with some degree of accuracy, identify and date them. Trade areas might eventually be worked out, both from the specimens themselves and with the aid of historic material such as the records of the Hudson Bay Company. The distribution of beads, trade cloths and ornaments suggest a general northern trade area in apparent sharp contrast to eastern and southeastern United States trade sources.

Theoretically, presenting these data chronologically gives Plains beaded art and design another dimension. Many of the early studies had necessarily to follow a purely

timeless approach, or if the dynamic aspects of art and techniques were studied distributionally, the data of study were assumed contemporary. Wissler (1917) got at time sequence by trait analysis or "comparative reasoning." Our admittedly tentative results imply, we feel, that at least some degree of time perspective in the field of Plains beaded art can still be obtained from specimens available in the various museums.

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[2] Acknowledgment is due to the staffs of the following museums: The United States National Museum, American Museum of Natural History, Museum of the American Indian, Museum of the Plains Indian, Sioux Museum, North Dakota Historical Society, Milwaukee Public Museum and the Chicago Natural History Museum.

LETTER TO THE EDITOR

TSCHOPIK'S *Aymara of Chucuito, Peru. I, Magic.*

SIR:

May I endorse Holmberg's superendorsement of La Barre's review of Tschopik's *Aymara: I, Magic?*

I concur that it is a superb functional study of magic, and would add that I can think of none that better illustrates the relation of an aspect of a culture with the personality typical of that culture.

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